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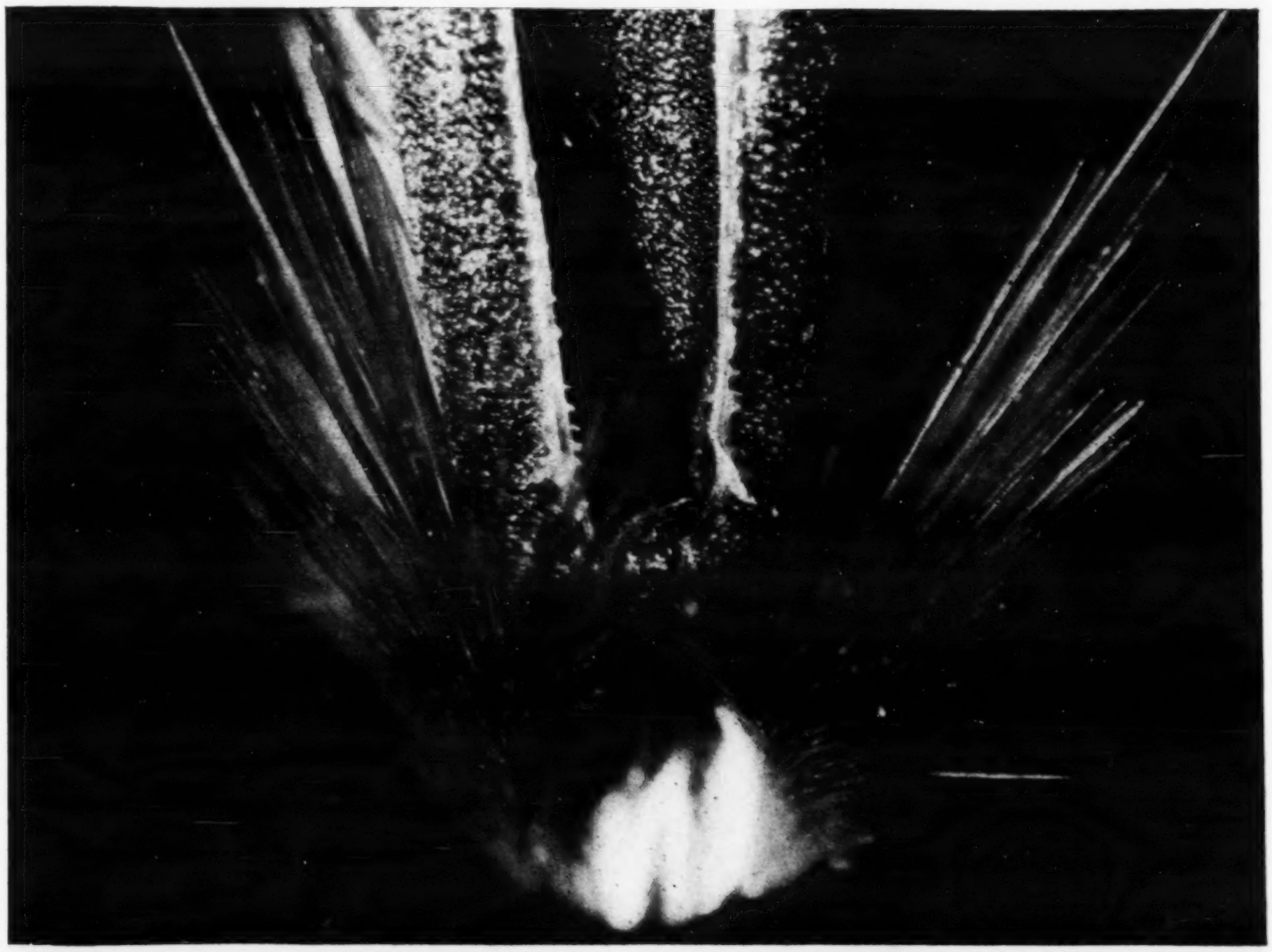

A Floret of Oats—Much Enlarged

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Comparative Fertility of the Only Child
Charles F. Stein

Consequences of Race-Crossing
Jon A. Mjoen—W. E. Castle

Hairless Mice

Mutations in the Sweet Potato

MAY, 1926 Vol. 17, No. 5

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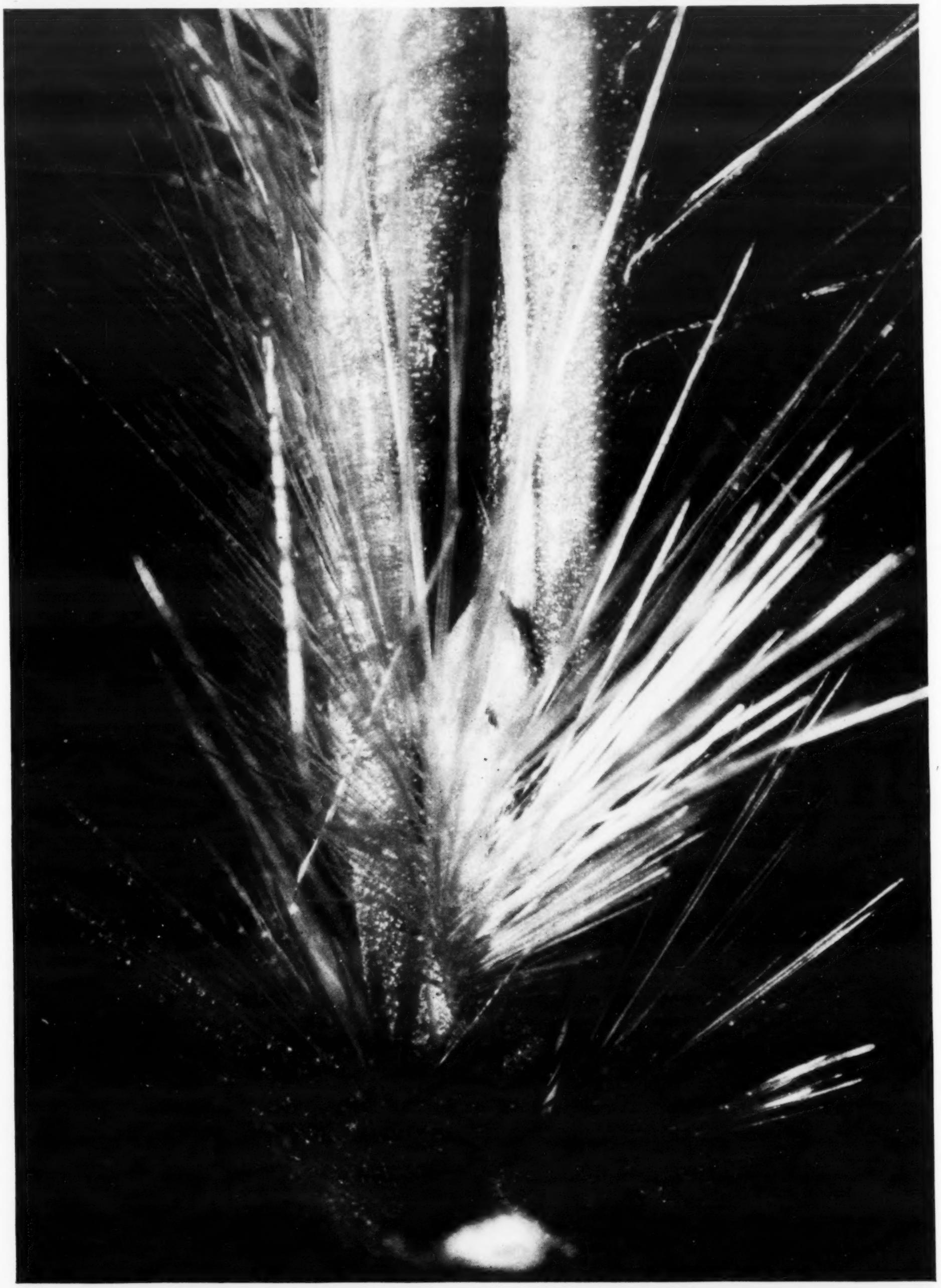


FLORET OF WILD OATS MUCH ENLARGED

Frontispiece

The common wild oat (*Avena fatua*) was probably native to central Europe or western Asia, although it has been spread by man to many other parts of the world. It is frequently found as a noxious weed. This species is supposed to be the progenitor of the cultivated forms of common oats. From these it differs chiefly in extreme hairiness, and in having a long spirally twisted and sharply bent awn. From the point of view of genetics and evolution it is interesting that a very similar form, the so-called "fatuoid or false wild oats" appears sporadically in cultivated oats. This form has the hairiness, the awns and the basal characters of *A. fatua*, but appears to be due to mutation or chromosome abnormality. In most varieties the complex of fatuoid characters is inherited in simple Mendelian fashion.

FATUOID OR FALSE WILD FORMS IN FULGHUM AND OTHER OAT VARIETIES

T. R. STANTON, F. A. COFFMAN, and G. A. WIEBE,
U. S. Department of Agriculture, Washington, D. C.

WITHIN the past fifteen years considerable interest has been shown in the occurrence of the fatuoid or so-called "false wild" form in cultivated oat varieties. Reports by many authors show that these forms have been observed in numerous varieties and in many different parts of the world. For several years the writers have been especially interested in the rather frequent occurrence, and in the breeding behavior, of fatuoids in Fulghum. Although more attention has been given to the study of such forms in Fulghum, fatuoids have been observed in the following additional varieties: Aurora, Burt, Cornellian, Culberson, Naked (hull-less), Navarro (Ferguson Navarro), Ruakura, Sixty-Day, Swedish Select, and Victory. Some of these have been studied genetically. Fatuoid forms also have been observed and selected from among progeny of several different hybrids grown at the Arlington Experiment Farm, Rosslyn, Va., but these are not discussed in this paper.

Review of Literature*

In the United States, progress in the study of these forms has not kept pace wth that in Europe. Oat forms, apparently very similar to those now referred to as fatuoids or false wild, were observed and studied in Europe more than forty years ago. About fifteen or twenty years ago considerable interest in these forms was shown in Europe and in Canada. In the United States, however, experiments to determine the origin and nature of fatuoids extend but slightly beyond the past five years.

A brief presentation of conclusions resulting from the more important genetic investigations of these forms is believed desirable. In general, writers agree in attributing their occurrence to one or the other of two causes: (1) loss-mutation, or (2) natural crossing between the wild oat, *Avena fatua* L., and the cultivated oat, *A. sativa* L. The literature reviewed indicates that false wild forms occur in many cultivated varieties, and an attempt has been made in this paper to present a comparatively complete list (Table I) of the different oat varieties in which such forms have been reported.

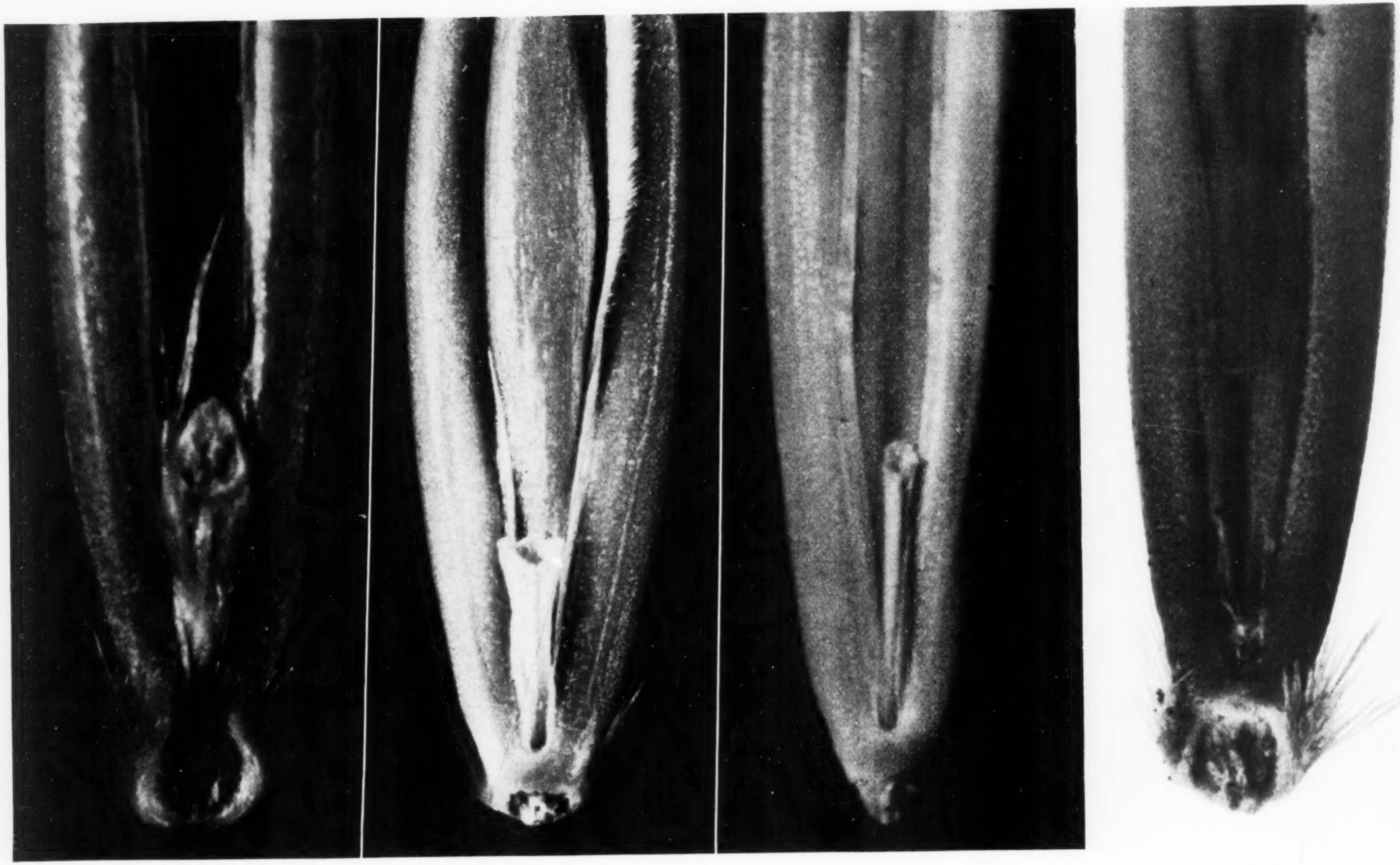
Genetics of Fatuoids

Koernicke and Werner[18]† as early as 1885 reported studies of oat forms which apparently they believed to have resulted from natural crossing between the wild oat, *Avena fatua*, and the cultivated oat, *A. sativa*. They also cited Haussknecht[16] who observed similar forms in Thuringia, which he termed "Zwischenformen." The descriptions presented by these authors give some reason for suspecting, as apparently Zade[30] likewise suspected, that the forms

†For numbered references see "Literature Cited" at the end of this article, to be published in the June Number.

*Since this manuscript was prepared a brief article on the occurrence of false wilds in Kanota and other varieties at the Kansas Agricultural Experiment Station has appeared in the Director's Report for the Biennium 1922-1924, pp. 38-41, inclusive.

An article entitled "The Origin of False Wild Oats," by C. L. Huskins and J. R. Fryer, also appears in *Scientific Agriculture* (*La Revue Agronomique Canadienne*), Vol. VI, No. 1, pp. 1-13, September, 1925.



Homozygous=fatuoid | Heterozygous=cultivated | Sativa | Byzantina

FLORETS OF CULTIVATED OATS AND FATUOID FORMS

Figure 1

The cultivated forms of *A. sativa* and of *A. byzantina* are shown at the right, and typical florets of a "homozygous-fatuoid" and of a "heterozygous-cultivated" (the "heterozygous false wild" form of other authors) are shown at the left. The heterozygous-cultivated form is usually heterozygous in breeding behavior, but not always so.

observed by Koernicke and Werner and those described by Haussknecht were similar to if not identical with those now called fatuoid or false wild.

Fischer[12] probably was one of the first to conduct extended experiments with these oat forms. As far as the present writers have been able to determine, he was the first investigator to point out physiological differentiating characters between these forms and the common wild oat, *Avena fatua.* He called these forms "Flughaferformen" in contrast to "Wildhafer," or the common wild. He observed that these forms would germinate in the fall, while seed of true *A. fatua* remained dormant in winter and did not germinate until the following spring. He believed this character precluded danger of the fatuoid becoming such a noxious weed as is the true wild oat.

Fischer apparently was not the first to recognize or to study these forms, but probably was the first worker to state that these forms were not closely related to true wild oats. Evidently he seriously questioned that the origin of these forms in winter oats could be explained on the basis of natural crossing. He observed that these forms appeared "spontaneously and quickly among the weakly haired and awned, gray-kerneled winter oat," a variety apparently identical with the Winter Turf or Virginia Gray oat.

The experiments of Nilsson-Ehle[22,23] which appear to have been started at about the time the results of Fischer's experiments were published, are among the most extensive investigations of the fatuoid or false wild oats yet reported. Nilsson-Ehle probably was the first to designate this oat form as "fatuoid," and doubtless was among the first to state specifically that its origin probably resulted from loss-mutation.

During the period from 1909 to 1912, inclusive, Criddle[6,7,8,9] presented several papers containing his observations and the results of his extensive experiments on these oat forms. He perhaps was among the first, if not the first, to designate these forms by the English terms "false wild." He described them as such in an address delivered before the Canadian Seed Growers' Association at Ottawa, Canada, in the winter of 1909-1910.

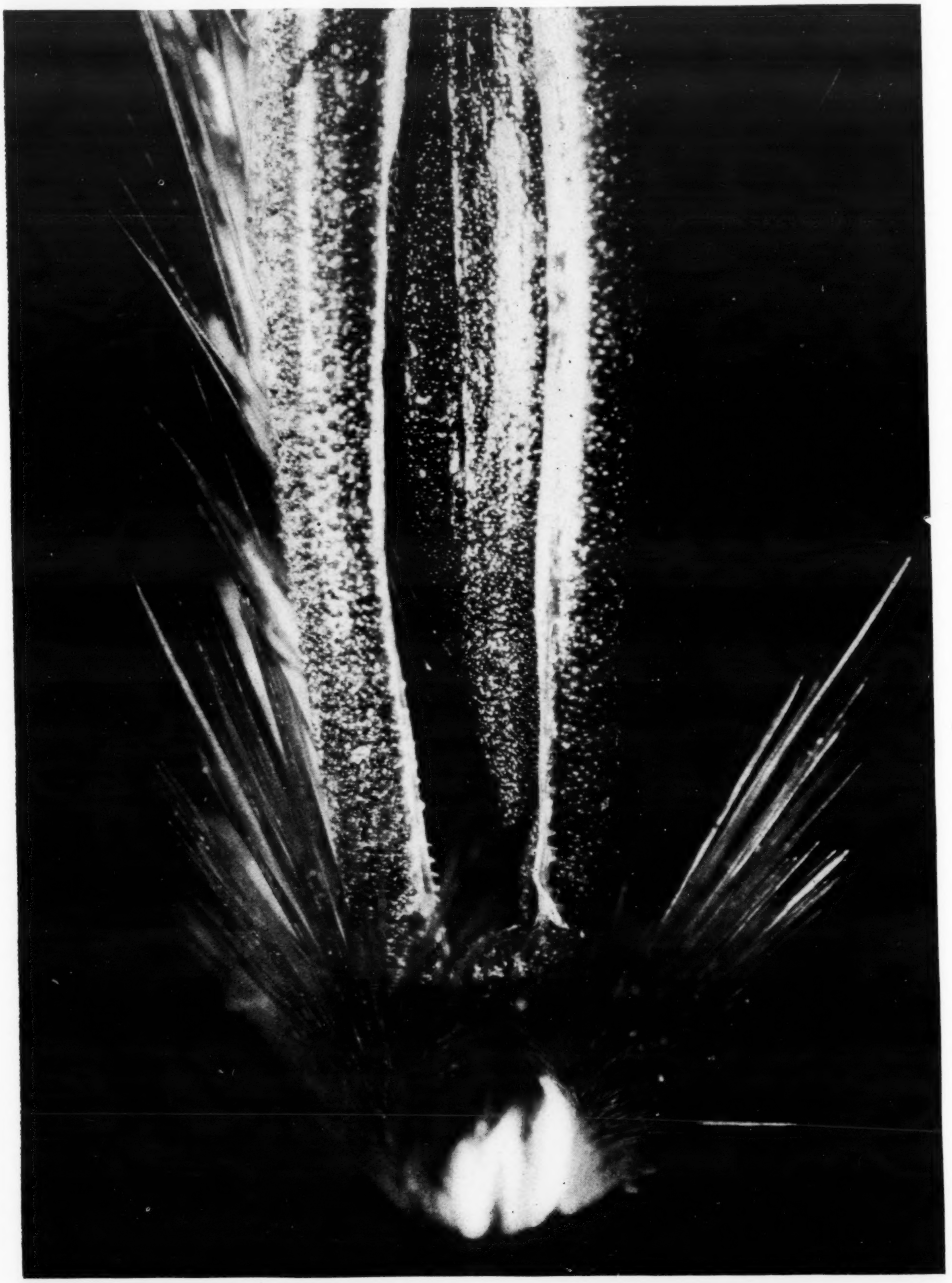
He arrived at practically the same conclusion regarding the origin of these forms as did Nilsson-Ehle, although he expressed his opinion in slightly different words.

In discussing the probable origin of false wild oats Criddle[7] states: "It is difficult to arrive at a satisfactory theory as to the cause of these sports; but, granting that the original type, from which cultivated oats were first selected, was heavily awned and had the basal characters of *Avena fatua,* we might surmise that they are retrogressive in the features noted to the original progenitor of present-day oats.

"It seems strange, however, that this probable form of atavism should be active in all or nearly all breeds of oats, and that both *Avena sativa* and *A. orientalis* should be equally active in producing sports." He further states: "I have not attempted to go into the matter of the true nature or cause of what I have termed sports in this paper, as to whether they have relation to the DeVries mutation, or whether any other factor or hypotheses are able to explain them." Criddle[9] also observed, as did Fischer, that false wild or fatuoid and true wild oats differ markedly in that the former will germinate in the fall, while the true wild oat, *A. fatua,* has the character of delayed germination.

An interesting sidelight on the history of studies of fatuoid or false wild oats in Canada is given by Criddle[8]. He stated in 1910: "The late Dr. Fetcher received specimens at least ten years ago and made numerous experiments which, I believe, led him to the conclusion that they were not true wild oats. Unfortunately, the time at his disposal prevented him from conclud-

The cultivated forms of *A. sativa* and of *A. byzantina* are shown at the right, and typical florets of a homozygous-fatuoid and of a "heterozygous-cultivated" (the "heterozygous false wild" form of other authors) are shown at the left. The heterozygous-cultivated form is usually heterozygous in breeding behavior, but not always so.



FLORET OF WILD RED OATS

Figure 2

This species (*Avena sterilis*) is believed to be the progenitor of such varieties as Burt and Fulghum. Genetically the fatuoid forms in these varieties behave differently from those in the supposed derivatives of *A. fatua,* but curiously enough these fatuoids resemble *fatua* rather than *sterilis.*

ing the work and much of the data collected was lost through his death."

Cooper[4], and Dow[10] in general corroborate the observations of Criddle regarding the nature and probable origin of the false wild oat in Canada.

Zade[29,30] made extensive experiments and observations on what he, following Haussknecht, calls "Zwischenformen" or forms intermediate between the wild oat, *Avena fatua,* and the cultivated oat, *A. sativa.* Therefore he concluded that these forms resulted from natural crossing rather than from loss-mutation. Tschermak[28] apparently arrived at a similar conclusion as the result of his experiments.

Zade and Tschermak, of all investigators who have devoted any considerable time to the study of fatuoid forms probably are the leading exponents of the theory that natural crossing is a more logical explanation for the occurrence of fatuoid forms than loss-mutation.

Newman[20,21] and Hays and Garber[17] have summarized briefly the investigations with false wild or fatuoid forms, and during recent years several investigators have presented additional papers supplementing their previous reports. Zade[31] has summarized his views on "Zwischenformen" (called fatuoids by Nilsson-Ehle), reiterating his belief that natural crossing may explain their occurrence, and Nilsson-Ehle[24] in a recent general report offers further argument in support of his idea that loss-mutation is the more logical explanation for the occurrence of these forms.

More recent writers on the subject, with but few exceptions, have favored the theory of loss-mutation as the more logical explanation for the occurrence of the fatuoid or false wild oat.

Crepin[5] reports an apparent fatuoid, the occurrence of which he attributes to degeneration in the cultivated oat, *Avena sativa.*

Akerman[1] reports the occurrence of fatuoids ("Fatuoiderna") or false wilds in pure lines, and also in mixed populations. He states that in addition to these fatuoid forms, other forms evidently of hybrid origin (heterozygotes) were occasionally seen, these forms naturally showing greater resemblance to the normal type. He observed, as had some previous investigators, that progeny of these heterozygotes segregate into "normal," "heterozygous," and "fatuoid" forms in the ratio of 1:2:1. He states that these aberrant forms apparently do not arise as the result of natural crossing between the cultivated and the common wild oat, *Avena fatua,* as the latter is not found in the Svalöf district of Sweden. He concludes that if a hybrid origin is excluded, the fatuoid must be regarded as a true mutant.

Marquand[19] describes fatuoid or false wild forms which he observed in several oat varieties. He states that these forms were bred in each case for two or three generations at Aberystwyth, Wales, and none segregated.

Garber[14] reports the finding of false wilds in several varieties of *Avena sativa.* In considering the origin of these aberrants he discusses the genetic behavior of certain F_2 generations of *A. fatua* × *A. sativa,* and *A. fatua* × *A. sativa orientalis.* He favors the explanation presented by Nilsson-Ehle and concludes that these forms may be attributed more logically to mutation than to natural crossing.

Garber and Quisenberry[15] have shown that delayed germination in crosses between *Avena sativa* and *A. fatua* is an inherited recessive character. In homozygous false wilds no delayed germination was found, although seed was used from a relatively small number of plants. This they believe is direct evidence against the hypothesis that natural crossing between *A. fatua* and *A. sativa* explains the origin of false wild forms. Further, they maintain that this refutation supports the theory that these same false wild forms arise as mutations.

Pridham[25,26] describes an oat form in the Mulga variety as being "slightly hairy and having the horse-shoe attachment characteristic of the wild oat." Pridham further states: "The ear-to-row selections from Fulghum were grown at Cowra last year and some bred true, while others showed considerable variation, suggesting the effect of natural crossing." Some of these unusual forms observed in both varieties by Pridham may have been false wilds or fatuoids.

Prevalence of Fatuoids

Fatuoid or false wild forms have been observed in many different cultivated oat varieties. In Table I the writers have listed the varieties in which such forms have been reported. The list is believed to be nearly complete. As far as possible, reference is made to the person who apparently first reported discovery of fatuoids in the variety and the locality in which the observation was made. In preparing this material for presentation it has been found that the results of most experiments with fatuoids have been published in foreign languages, and exceedingly few translations into English are available.

Description of Fatuoid Forms

It has been observed that fatuoid or false wild forms usually possess many characters common to the variety in which they are found, but all such forms are similar to each other in many general characters, regardless of the variety in which they occur. The more striking characters of the fatuoid kernel are a prominent basal cavity, dense and often very long hairs or bristles on the base of the lemma as well as on the supported rachilla segment, and a long twisted and geniculate awn on all florets of the spikelet. When mature the fatuoid usually shatters very readily, making it extremely difficult to find panicles containing many kernels when the plants have reached full maturity in the field. Often the secondary kernels of the spikelet of the fatuoid (homozygous) are first to shatter or fall.

Fatuoids with the above characters have been genetically homozygous or have bred true in the varieties in which these forms have been studied previously. However, in Fulghum and Burt, two of the varieties included in the present investigations, several of these supposedly homozygous forms have failed to breed true.

In investigations of previous writers, plants bearing spikelets with a twisted and geniculate awn on the lower floret only, in which spikelet separation occurs by semiabscission resulting in a slight basal cavity, and floret disjunction or separation by disarticulation as in typical *Avena sativa*, and having the callus often only sparsely hairy, were heterozygous in genetic behavior as a rule, and usually have been referred to as "heterozygous false wilds." In the studies here reported it has not been possible always to differentiate phenotypically between the genotypically heterozygous false wild and homozygous cultivated forms. In discussing these aberrant forms in this paper, the following terminology is used to avoid confusion. The truly homozygous fatuoid or false wild is termed *homozygous-fatuoid*. The second fatuoid form which phenotypically appears to be identical with the homozygous fatuoid, but is genotypically heterozygous, is termed *heterozygous-fatuoid*. The so-called heterozygous false wild of most previous writers, is termed *heterozygous-cultivated*. The genotypically homozygous normal is called *homozygous-cultivated*. The basal characters of florets of the homozygous-fatuoid or heterozygous-fatuoid, and heterozygous-cultivated (intermediate) forms, are shown in Figure 1. Typical florets of *Avena sterilis*, the wild red oat, and *A. fatua*, the common wild oat, are shown in Figure 2, and the frontispiece, respectively. Florets of the cultivated derivatives, *A. byzantina*, the red oat, and *A. sativa* the common oat, are shown in Figure 1.

Experimental Data*

The writers have described fatuoid forms in 11 oat varieties, including Fulghum, Burt, Navarro (Ferguson Navarro), Aurora, Culberson, Sixty-Day, Swedish Select, Ruakura, Victory, Cornellian, and Naked (hull-less). The experiments reported were conducted at three widely separated points in the United States. As previously stated, more time has been given to the study of fatuoid forms from Fulghum than from all other varieties. The occurrence and study of these forms will be discussed for each variety separately.

Fulghum

In 1919, 11 fatuoid or false wild plants were selected from plats of the Fulghum variety at the Arlington Experiment Farm. Such forms were observed in five different strains of that variety. Two of these strains had been grown at that station for the first time the previous season, but the other three had been grown during periods varying from three to six years. One each of the two most recent accessions of the variety had been received from the Kansas and South Carolina Agricultural Experiment Stations, respectively. The occurrence of fatuoids in strains from sources so widely separated indicated that their presence in Fulghum had not resulted from mechanical mixture.

During the 5-year period from 1920 to 1924, inclusive, numerous nursery sowings of fatuoids were made at the Aberdeen Substation in order to obtain information on the genetic nature of such forms. Fatuoids from Fulghum were divided according to their source into two groups: (1) those from mass lots; and (2) those originating from a single plant selection which, however, according to the experimental results presented herein, may not necessarily have been genetically pure.

Forms From Mass Lots

In 1920, a head row of each of the 11 aberrants found at the Arlington Experiment Farm in 1919 was sown at the Aberdeen (Idaho) Substation. Descriptions of the parental plants were not recorded but their breeding behavior indicated that genotypically 4 were homozygous-fatuoid, 4 were heterozygous-fatuoid, and 3 were heterozygous-cultivated. The 4 homozygous-fatuoid plants produced 83 progeny, all of which were like the parents. The 4 heterozygous-fatuoid plants produced 55 progeny, 48 of which were classed as homozygous-fatuoid or heterozygous-fatuoid,† and 7 as homozygous-cultivated or heterozygous-cultivated. The 3 heterozygous-cultivated plants produced 48 progeny of which 13 were classed as homozygous-fatuoid

*Selection of the original fatuoid plants from the Fulghum, Aurora, Culberson, and Swedish Select varieties, and the conducting of experiments with them, has been the work of the senior writer. Experiments with the forms from Burt, Sixty-Day, and Ruakura were conducted by F. A. Coffman, who selected the fatuoids from these varieties. In 1923, three fatuoid plants were selected from Navarro (Ferguson Navarro), one each by John W. Taylor and the senior writer at the Arlington Experiment Farm, Rosslyn, Va., and one by F. A. Coffman at Akron Field Station, Akron, Colo. Fatuoids in the variety Victory were found in 1924 by the senior writer in a commercial field in western New York, and by G. A. Wiebe in an experimental plat at the Aberdeen Substation, Aberdeen, Idaho. Those in Cornellian were selected in 1924 by members of the Plant Breeding Department at Cornell University, Ithaca, N. Y. The Naked (hull-less) false wild was found by Matthew Fowlds at the Agricultural Experiment Station, Brookings, S. Dak., in 1924. Some of the data were obtained at Akron Field Station, Akron, Colo., but most of the results presented in this paper were recorded on the Aberdeen Substation, Aberdeen, Idaho, where cereal breeding experiments are conducted under the immediate supervision of G. A. Wiebe.

†The reader must keep in mind the fact that in the Fulghum and Burt varieties phenotypically the homozygous-fatuoid and heterozygous-fatuoid apparently are identical. They differ genotypically, however, which can be determined only by growing progenies from them. Likewise, it is not possible always to differentiate phenotypically between the heterozygous-cultivated and homozygous-cultivated forms.

or heterozygous-fatuoid, and 35 as homozygous-cultivated or heterozygous-cultivated.

The ratio of homozygous-fatuoid or heterozygous-fatuoid to homozygous-cultivated or heterozygous-cultivated forms in the progeny of the 3 heterozygous-cultivated parents was such as to suggest the Mendelian ratio of 1 to 3, a ratio, however, which has not been confirmed by further experimentation with larger numbers of individuals.

All strains of Fulghum oats were almost completely winterkilled at the Arlington Experiment Farm in 1920 and, as a result, no additional fatuoids were obtained for studies in 1921.

During the summer of 1921, a second lot comprising 18 aberrant plants was selected from the varietal plats of Fulghum at the Arlington Experiment Farm. The seed from these plants was sown in individual rows at the Aberdeen Substation in 1922. Of these 18 plants, 14 were classed as homozygous-fatuoid or heterozygous-fatuoid, and 4 as homozygous-cultivated or heterozygous-cultivated forms. Of the 14 plants classed as homozygous-fatuoid or heterozygous-fatuoid, 11 bred true, producing 179 progeny plants, all typical of the parental form, while among the progeny of the other 3 segregation occurred even though phenotypically they were apparently identical with the homozygous-fatuoid plants. These three heterozygous fatuoids produced segregating progeny as follows: 34, 14, and 23 of the parental form, and 3, 1, and 1 of the homozygous-cultivated or heterozygous-cultivated form, respectively.

The breeding behavior of the four parental plants classed as homozygous-cultivated or heterozygous-cultivated from which progenies were grown at the Aberdeen Substation in 1922 was varied. One plant produced 25 progeny which showed no segregation but produced an interesting result in that all progeny were described as homozygous-fatuoid or heterozygous-fatuoid. Two of the four produced a total of 18 progeny which did not segregate and all of which resembled the parents. The fourth heterozygous-cultivated parental plant produced 92 progeny, 20 of which were classed as homozygous-fatuoid or heterozygous-fatuoid, and 72 as homozygous-cultivated or heterozygous-cultivated.

The results obtained in 1920 at the Aberdeen Substation were not conclusive evidence that the fatuoid forms in Fulghum, that is, those that must be classed phenotypically as homozygous-fatuoid, are not always genotypically homozygous. Detailed descriptions of each of the parental forms of the progeny grown in 1920 had not been recorded. The results obtained in 1922, however, were more definite proof that the forms in this variety which previous investigators usually have considered as homozygous fatuoids or false wilds, are not necessarily always homozygous. To the authors' knowledge, this fact has not been previously observed by investigators who have studied these aberrant forms in other cultivated oat varieties. Also, the results of 1922 proved that the breeding behavior of the plants classed as homozygous-cultivated forms in Fulghum differs from that of such forms in other varieties and apparently is very complex and difficult of explanation.

Because of the complexity of inheritance the study of fatuoid plants from unselected Fulghum oats was continued in 1923. Thirty plant rows sown from seed of aberrant plants selected at the Arlington Experiment Farm in 1922, and classed phenotypically as homozygous fatuoids, were grown at the Aberdeen Substation in 1923. Only 11 of these 30 plants bred true. These produced a total of 190 progeny plants. Each of the remaining 19 parental plants produced some progeny of the homozygous-cultivated or heterozygous-cultivated forms.

From the 19 heterozygous-fatuoid parental plants, which for all readily observable phenotypic characters were identical with the homozygous-fatuoid forms but proved to be genotypically heterozygous, a total of 309 progeny plants was grown. Of these, 271 were classed as being homozygous-fatuoid or heterozygous-fatuoid, and 38 as homozygous-cultivated or heterozygous-cultivated. It is of special interest to note that in most cases only one or two progeny plants classed as homozygous-cultivated or heterozygous-cultivated, were produced from each parental plant, a preponderance of all the progeny being classed as homozygous-fatuoid or heterozygous-fatuoid.

From the results obtained it does not seem that the 11 apparently genotypically homozygous-fatuoid plants which produced no homozygous-cultivated or heterozygous-cultivated forms among their progeny were necessarily homozygous. On the basis of the data obtained, it is very likely that larger populations from these 11 plants might have contained some homozygous-cultivated or heterozygous-cultivated plants. In this connection it is interesting to recall that some of the progenies from heterozygous-cultivated parent plants have contained only a very few of the forms classed as homozygous-fatuoid or heterozygous-fatuoid, while other parent plants of apparently the same form have produced numerous progeny which phenotypically were 100 per cent homozygous-fatuoid. The production from heterozygous-fatuoid parental forms of one or two plants classed as homozygous-cultivated or heterozygous-cultivated in progenies of from 15 to 20 plants, and the occurrence in similar sized progenies from heterozygous-cultivated parental forms of one or two plants classed as homozygous-fatuoid or heterozygous-fatuoid, certainly indicate genetic ratios other than those previously reported.

In addition to the aberrants described above three Fulghum fatuoid plants with dark brown kernels were found at the Arlington Experiment Farm in 1922. Progenies were grown from each of these at the Aberdeen Substation in 1923. A total of 55 homozygous-fatuoid plants was produced, all of which were like their respective parents. These fatuoid forms with dark brown kernels were taller and later in maturing than the usual or red-kerneled fatuoids found in Fulghum.

Seed of each of 72 plants classed as homozygous-cultivated or heterozygous-cultivated, which were grown from a single heterozygous-cultivated plant in the experiments with fatuoids from mass lots of Fulghum in 1922, was sown in plant rows at the Aberdeen Substation in 1923. Segregation occurred in the progeny of 46 of these parental plants, a total of 1,033 progeny plants being produced, of which 270 were described as homozygous-fatuoid or heterozygous-fatuoid, and 763 as homozygous-cultivated or heterozygous-cultivated, the latter being similar to the parental type. Of the total of 72 parental individuals, 26 produced progenies which did not segregate, all resembling the homozygous-cultivated form of Fulghum.

In 1923, progenies were grown from 7 plants classed as homozygous-fatuoid or heterozygous-fatuoid at the Akron Field Station. Three of these 7 aberrants produced a total of 29 progeny, all of which resembled the parental form in all readily observable characters. Three others of the aberrants produced a total of 22 progeny plants classed as homozygous-fatuoid or heterozygous-fatuoid, and 3 progeny plants classed as homozygous-cultivated or heterozygous-cultivated, each of these 3 parental aberrants producing one homozygous-cultivated or heterozygous-cultivated segregate. The seventh aberrant produced 10 progeny, 2 of which appeared to be like the parent, and 8 were classed as homozygous-cultivated or heterozygous-cultivated.

These results were similar to those obtained at the Aberdeen Substation, and further demonstrated that the homozygous-fatuoid phenotypes in Fulghum are not necessarily genetically pure but may be unstable in breeding behavior.

Forms from Selected Lines

Wide segregation occurred in the progeny of a plant selection of Fulghum, C.I. No. 708-209* grown at the Arlington Experiment Farm in 1921. The parent plant, seed of which was sown in this row, exhibited no characters sufficiently striking to make the plant especially noticeable among others of the variety used in sowing similar plant rows in the fall of 1920. In the head row grown in 1921, 39 plants were produced, 11 of which were classed as homozygous-fatuoid or heterozygous-fatuoid and 28 as more closely resembling the homozygous-cultivated form, or that of the parental plant. The data were too meager to indicate the nature of genetic factors responsible for the production of these aberrant forms in the Fulghum variety, and the experiments were continued in 1922.

Progenies from each of the 39 plants from the 1921 plant row were grown in 1922. The data obtained are shown in Table II. These data are sufficiently extensive to indicate that the occurrence of these aberrant forms in the Fulghum variety can not be explained on a single factor difference, such as is believed to exist between fatuoid aberrants and the normal or cultivated forms in many other varieties. Of the 11 parental plants described as homozygous-fatuoid or heterozygous-fatuoid, 9 produced progeny only of that type. The other two produced progeny showing segregation as follows: Homozygous-fatuoid or heterozygous-fatuoid, 29; homozygous-cultivated or heterozygous-cultivated forms, 4. Of the 28 parental plants described as homozygous-cultivated or heterozygous-cultivated, 11 produced segregating progeny, 41 plants being classed as homozygous-fatuoid or heterozygous-fatuoid and 215 of the parental form. The other 17 of these 28 homozygous or heterozygous parents produced progeny which showed no segregation, all the plants being of the parental phenotype.

Because of limited space only one of the 13 segregating families of 1922 was grown at Aberdeen in 1923. This particular family had been classed as producing 4 homozygous-fatuoid or heterozygous-fatuoid, and 19 homozygous-cultivated or heterozygous-cultivated plants. Seventeen of these latter plants exhibited almost a complete absence of awns, and there seemed reason for believing that they were normal, cultivated individuals. The kernels of the other two plants bore awns on the lower florets and some basal hairs. The breeding behavior of the 23 plants of this family, as shown by the data in Table III, indicated that very few individuals of either phenotype were genotypically homozygous, and that homozygosity can not be assumed on the basis of the phenotype in this variety. Only 4 of the 17 parent plants believed to be genetically homozygous for the cultivated form showed no segregation in their progeny, and the progeny of only 1 of the 4 supposedly homozygous fatuoids did not segregate. The data therefore are considered as sufficient evidence for believing that the inheritance of the fatuoid form in the Fulghum variety depends upon several factors.

In 1924, further study of fatuoid forms in pedigreed lines of Fulghum was made at the Aberdeen Substation. Populations larger than in previous years were grown from each of 5 parent plants classed as homozygous-fatuoid or heterozygous-fatuoid, and from each of 5 classed as homozygous-cultivated or heterozygous-cultivated.

*The number 209 represents the ninth plant selection made in 1920 from Fulghum, C. I. No. 708.

The progeny of 3 of the 5 aberrants segregated, producing a total of 115 progeny plants of the parent type and 7 plants of the homozygous-cultivated or heterozygous-cultivated type. The 2 remaining aberrants produced, respectively, 82 and 88 progeny all similar to the parental form. This latter result may indicate that genotypically homozygous fatuoids exist in the Fulghum variety.

A total of 388 progeny was grown from the 5 parental plants classed as homozygous-cultivated or heterozygous-cultivated. Segregation occurred among the progeny of all 5 plants. A total of 68 plants classed as homozygous-fatuoid or heterozygous-fatuoid, and 320 plants classed as homozygous-cultivated or heterozygous-cultivated, resulted. The segregation apparently indicates no definite genetic ratio.

In the course of these experiments nearly 4,500 plants of Fulghum have been grown, a very large percentage of which have been the progeny of parent plants which on the basis of phenotype would have been classed as truly homozygous fatuoids. The data presented indicate that some of these supposedly homozygous-fatuoid and some of the supposedly homozygous-cultivated forms in Fulghum are genotypically homozygous, yet they also show that many other individuals of the same phenotypes are heterozygous in their breeding behavior. The data from these experiments, therefore, differ markedly from those obtained by others who have studied the breeding behavior of the fatuoid forms only in varieties of *Avena sativa*. Usually it has been shown that the fatuoid differs most probably by a single factor from the cultivated form in which it was observed. The results obtained in these investigations are sufficient to indicate that at least some of the aberrants in Fulghum are dependent upon several factors. The data do not, however, indicate the actual number of factors involved.

(Data on Fatuoids in other varieties, and the discussion will appear in the June number)

TABLE I. VARIETIES OF AVENA SATIVA AND A. SATIVA ORIENTALIS IN WHICH FATUOIDS HAVE BEEN OBSERVED, AND NAME AND LOCATION OF FIRST RECORDED OBSERVER

*Variety or Selection**	*Location*	*Author*
Gray Winter	Saxony, Germany	Fischer
Banner	Manitoba, Canada	Criddle
Storm King	" "	"
Newmarket	" "	"
Abundance	" "	"
Bumper King	" "	"
0197 Probsteier × Black Tartarian	Svalöf, Sweden	Nilsson-Ehle
0201 Select. Black Tartarian	" "	"
0310 " Probesteier	" "	"
0311 " "	" "	"
0327 " "	" "	"
0408 Black Glocken Oats × 0450	" "	"
Black Great Mogul	" "	"
01051 Select. Black Tartarian	" "	"
01055 " Black Swedish	" "	"
01059 " " "	" "	"
01060 " " "	" "	"
0301 Hvitling	" "	"
0355 Victory	" "	"
Vilmorin's Ligowa	" "	"
Danish	" "	"
Old Island Black	Manitoba, Canada	Criddle

*Varieties arranged chronologically in accordance with the reports of the observations of fatuoids in them.

*Variety or Selection**	*Location*	*Author*
Black (Unnamed)	Manitoba, Canada	Criddle
Swedish Select	" "	"
Irish Victor	" "	†"
Early Ripe	" "	"
Golden Rain	——— France (?)	Crepin
Black Winter	Wales, British Isles	Marquand
Supreme	" "	"
Sir Douglas Haig	" "	"
Aurora	Minnesota, U. S. A.	Garber
Garton 784	" "	"

*See footnote under Table I, p. 163.
†Editor's note in publication by Criddle (9) in 1912.

TABLE II. GENETIC SEGREGATION OF THE PROGENIES FROM THE 39 PARENTAL PLANTS OF THE ORIGINAL PLANT SELECTION OF FULGHUM, C. I. NO. 708-209, GROWN AT ABERDEEN, IDAHO, IN 1922.

Row No.	Parent phenotype	Phenotypic classification of progeny: Homozygous-fatuoid	Phenotypic classification of progeny: Homozygous-cultivated or heterozygous-cultivated	Row No.	Parent phenotype	Phenotypic classification of progeny: Homozygous-fatuoid	Phenotypic classification of progeny: Homozygous-cultivated or heterozygous-cultivated
654	Homozygous-fatuoid	21	0	671	Homozygous-cultivated	2	23
655	Do	19	0	672	Do	4	19
656	Do	22	0	673	Do	0	18
657	Do	9	0	674	Do	0	22
658	Do	22	0	675	Do	3	21
659	Do	18	0	676	Do	4	16
660	Do	19	1	677	Do	0	22
661	Do	10	3	678	Do	4	21
662	Do	20	0	679	Do	0	24
663	Do	13	0	680	Do	1	24
664	Do	18	0	681	Do	5	19
690	Heterozygous-cultivated	7	18	682	Do	0	26
691	Do	4	15	683	Do	0	25
692	Do	3	19	684	Do	0	25
665	Homozygous-cultivated	0	24	685	Do	0	24
666	Do	0	19	686	Do	4	20
667	Do	0	21	687	Do	0	21
668	Do	0	26	688	Do	0	20
669	Do	0	21	689	Do	0	25
670	Do	0	22				

Summary

Parent genotype as indicated by phenotype of progeny and number of progeny from each.

	Number Parent Plants	Phenotypic Classification of Progeny: Homozygous-fatuoid	Phenotypic Classification of Progeny: Homozygous-cultivated or heterozygous-cultivated
Homozygous-fatuoid	9	162	0
Heterozygous-fatuoid	2	29	4
Heterozygous-cultivated	11	41	215
Homozygous-cultivated	17	0	385

TABLE III. GENOTYPIC GROUPING OF PARENT PLANTS OF A HETEROZYGOUS-CULTIVATED FAMILY* OF 1922 FROM THE ORIGINAL PLANT SELECTION OF FULGHUM, C. I. NO. 708-209, ON THE BASIS OF SEGREGATION OF THEIR PROGENIES, GROWN AT THE ABERDEEN SUBSTATION IN 1923.

Row No.	Phenotypic classification of parent plants	Total number of plants per row	Classification of progeny phenotypically: Homozygous-fatuoid	Heterozygous-cultivated	Homozygous-cultivated	Total for homozygous-cultivated or heterozygous-cultivated
626	Homozygous-cultivated	19	1	11	7	18
628	Do	23	6	12	5	17
629	Do	23	4	12	7	19
630	Do	25	5	7	13	20
631	Do	21	3	13	5	18
632	Do	23	2	10	11	21
633	Do	23	7	9	7	16
636	Do	19	5	11	3	14
639	Do	25	3	7	15	22
640	Do	22	6	7	9	16
641	Do	24	8	12	4	16
642	Do	17	4	9	4	13
643	Heterozygous-cultivated	17	4	6	7	13
644	Do	22	4	9	9	18
Summary	for genotypic heterozygous-cultivated	303	62	135	106	241
627	Homozygous-cultivated	20	0	0	20	20
634	Do	23	0	0	23	23
635	Do	22	0	0	22	22
637	Do	18	0	8	10	18
638	Do	24	0	0	24	24
Summary	for genotypic homozygous-cultivated	107	0	8	99	107
645	Homozygous-fatuoid	21	20	1	0	1
646	Do	14	14	0	0	0
647	Do	11	7	4	0	4
648	Do	21	20	1	0	1
Summary	for homozygous-fatuoid	14	14	0	0	0
Summary	for heterozygous-fatuoid	53	47	6	0	6

*From row number 672 at Aberdeen in 1922, see Table I.

MUTANT ROOTS OF YELLOW JERSEY SWEET POTATO

Figure 3

A white-skinned root is shown at the left and at the right two views of a striped root. The white form is perpetuated by vegetative propagation and it seems probable that many of our sweet potato varieties originated in this way.

MUTATIONS IN THE SWEET POTATO

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OF THE many varieties of sweet potatoes grown in this country the exact origin of even one is not known. Since the sweet potato does not produce seed in the continental United States, with the possible exception of in Florida, the origin of our numerous and diverse cultivated forms is all the more interesting. It seems unlikely that so many varieties could have been introduced from the tropics, in the form in which we now grow them. Of possible importance in the development of new forms is the occurrence of mutations.

The writer on different occasions has observed the presence of white skinned roots in hills of the Red Bermuda variety, in Virginia and in Missouri. In 1924, a plant of the Yellow Jersey variety was found on the ranch of A. R. Vierra, Turlock, Calif., bearing two white roots, one root longitudinally striped with white and yellow, and several normal yellow in color. The latter were lost, as well as one of the white roots, but one white root and the striped one were preserved, and plants grown therefrom at Davis, Calif., on the Experiment Station Farm in 1925.

Three plants were obtained from the white part of the striped root, and these bore in their 1925 crop, all white potatoes. Six plants were grown from the yellow part of the same root and these produced all yellow roots like the parental variety. From the potato white all over, ten plants were grown, all of which bore white roots.

The yellow skin color of sweet potatoes is due to the presence of pigment in the epidermal cells, and this is absent in the white potatoes. However, the sprouts, by which the sweet potato is vegetatively propagated, arise from the cambium. Hence the original striped potato must be considered as a sectorial chimera, since more fundamental tissues were involved than the epidermal cells alone. The roots entirely white probably originated from a single mutant cell.

In the crop from the mutant plants, the plants having white roots were inferior in yield to their yellow brethren, as shown in Table I.

It appears that the mutation has involved the yielding power in some way. Since it is unlikely that the change in color, in itself, would alter the crop-producing ability of the plants, it is probable that other factors were involved in the mutation.

The potatoes produced by the white mutant plants, besides their smaller size, appeared to be more slender or stringy than the yellow tubers. To determine if the mutation involved factors for form of root, measurements were made of the greatest diameter and the length. Only potatoes having a diameter of 25 m.m. or over were measured, smaller roots being considered "strings" and not representative of tuber shape for the variety. The results are tabulated in Table II.

In both the white lots, the ratio of length to diameter is greater than in the yellow lot. The difference in one case is less than the probable error, but in the other the difference is 3.5 times the probable error. It cannot be said that a difference in form certainly exists in these mutant strains.

In the Nancy Hall variety, a hill was found at Turlock in 1924, bearing white potatoes, whereas the roots of this variety are normally more or less suffused with pink. Forty-two plants grown from this white Nancy Hall in

1925, produced thirty hills having white potatoes, and twelve of the usual color for this variety. Evidently the mutation did not involve the whole of the parent hill, or reversion to the parental type occurred.

TABLE I. Yield of White and of Yellow Sweet Potatoes

Origin of Plants	Wt. per hill	No. roots per hill	Av. wt. per tuber
From white potato	1,222 grams	23.3	52.3 grams
From white stripes	777 grams	16.6	46.6 grams
From yellow stripes	2,040 grams	22.2	92.2 grams

TABLE II. Measurements of Roots of Sweet Potato Mutants

Origin	No.	Mean length	Mean diameter	Mean ratio L/D	Difference from yellow
From white potato	78	129.6 m.m.	38.6	3.634±.075	.101±.149
From white stripes	18	140.0 m.m.	37.0	3.933±.123	.400±.113
From yellow stripes	69	133.2 m.m.	40.1	3.533±.085	

Pseudo-Science by a Pseudo-Scientist

THE NEW AGE OF FAITH *by* JOHN LANGDON-DAVIES. Price $2.50. The Viking Press, New York, 1925.

Langdon-Davies is a paradox. He believes in evolution but not in heredity. Furthermore he has no use for theology and less for eugenics. The "Heredity Fiend" and the "Race Fiend" are even worse than the fundamentalist because they ought to know better. "Finally we should remember that, as deep calls to deep, so too, stupidity calls to stupidity, and in this New Age of Faith it is the false prophets of pseudo-science who make possible by their manifold error the errors of anti-science."

Parts of the book are excellent, but we must always bear in mind that the author is a pseudo-scientist himself. He chooses the facts that support his particular views; facts of an opposite implication are either ignored or are disposed of as being "unscientific." His plea against "fiends" of one kind or another would have been more convincing had he been satisfied to stop there, instead of trying to foist on the reader his own particular brand of pseudo-science.

It is surprising that one who presents the "Truth about Evolution" in such excellent form should be so amazingly blind as to the importance of heredity. "We know that the naked baby is the same whether its social milieu is to be Athens or Chicago. We can state emphatically that, though the small differences between men may be due to heredity, the great differences, the important differences are due to what the social milieu does to the little differences." He produces no conclusive evidence that would lead us to believe anything of the kind. In a book devoted to the demolition of the "pseudo" such statements unsupported either by common sense or by scientific observation, are particularly edifying. Langdon-Davies may have a case but he does not come into court with clean hands.

R. C.

COMPARATIVE FERTILITY OF THE ONLY CHILD

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SIR Francis Galton, in his *Hereditary Genius* expresses the view that the prime cause of the extinction of noble families in England has been marriage with heiresses. He observed that there was a strong tendency on the part of the English peers to marry women who would inherit wealth. Such a woman, being the only child of her parents (otherwise under English law their fortune would not have passed solely to her) and coming therefore from relatively unprolific stock, was in turn herself likely to be unprolific, and thus, in many cases, the noble line came to an end. To quote Galton's own words:

> * * * these marriages are peculiarly unprolific. We might indeed have suspected that an heiress, who is the sole issue of a marriage, would not be as prolific as the woman who has brothers and sisters. Comparative fertility must be hereditary in the same way as other physical attributes, and I am assured it is so in the case of the domesticated animals.

This theory of Galton's does not ever seem to have received the attention its importance deserves. Students of human hereditary have given little notice to the question of comparative fertility, and writers on eugenics or allied topics frequently ignore it altogether.

Although certain German investigators have endeavored to show that heiresses[1] are more often sterile than other women, the general view in the medical profession, at least, seems to be to the contrary. Thus Max Huhner, in his *Sterility in the Male and Female* says—that he finds no evidence of such a tendency because the women who came to him for treatment for sterility were from families averaging nine children. But it is scarcely necessary to point out the obvious fallacy in such reasoning. The point under consideration is—not, what percentage of sterile women are "heiresses," but what percentage of "heiresses" are sterile, and how does this compare with the frequency of sterility in the "average woman." Perhaps none of Huhner's women came within the class under consideration. The only test is to collect data on a large number of "heiresses" and determine their average fertility, and then compare this with the fertility of the "average woman."

The difficulties of an investigation of this sort, aside from the difficulty of collecting the necessary number of cases, are considerable. Many factors non-inheritable in the woman may operate to cause failure of issue and in a purely statistical investigation it is impossible to inquire into the operating cause. For example, in an issueless marriage it may be the husband who is at fault rather than the wife, which Huhner believes to be the case in nearly one half of the instances, either by being at fault himself, or by communicating to the wife a disease rendering her sterile. Such circumstances, however, can be determined only by medical examination or knowledge of the history of the parties. Other factors are even more difficult of analysis. To cite an example from History, the marriage of Henry VIII of England to Catherine of Aragon resulted, to all outward appearances in but one child, Mary Tudor,—which would lead the investigator to believe that Catherine was a relatively unprolific woman.

[1]For the sake of convenience the term "heiress" will be employed in this paper for any woman who is the sole issue of her parents no matter whether wealthy or poor, the biological factors being the same.

That this character was hereditary he might find corroborated in the fact that the marriage of Mary to Philip II of Spain was in turn issueless. The real facts are, however, that not one, but ten children were born to Henry and Catherine, of which as a result of the syphilis with which the royal pair were infected,[2] only Mary survived. The failure of the latter to have issue is explained by the fact that she lived with her husband but a few weeks, and not that she had inherited a congenital unprolificness from her mother.

Thus in certain cases the "heiress" appears to be the sole issue of her parents when in fact she is merely the only surviving child. Both Maria-Theresa and Isabella, who inherited respectively the thrones of Austria and Castile, had brothers who died in infancy. The former had ten children and the latter one. In other cases only one child may be born, not because of the barreness of the wife but because the marriage took place late in the life of the parties, or because the marriage was broken up by death or separation, as in the case of the marriage of Henry VIII and Jane Seymour. In such cases we would not expect the offspring to display "hereditary" sterility, but the inclusion of a number of individuals of this sort in a list of cases might lead to an erroneous conclusion.

It can readily be seen therefore that there are many factors that might operate to mislead the statistical investigator. He may assume however, that these are no more likely to operate in the case of "heiresses" than among other women, and would therefore not influence a conclusion based on a large number of cases, although they might render unreliable one based on but a few.

One other important factor remains to be considered, namely, voluntary limitation of offspring. The size of the family is regulated at the present day and no doubt in past times as well. Very few married couples, however, voluntarily forego offspring altogether, and especially strong in all societies has been the desire to have a son to inherit the family name, so that where the first child is a girl, it must be but rarely that the parents are not desirous of having another child in the hope of its being a son. We may therefore make the further assumption which should be true in dealing with a large number of cases, that where the family consists of but a single daughter, the explanation is something other than voluntary restriction of birth.

Since the result of infertility is the extinction of the family line, there is often difficulty in understanding how a tendency to infertility can be "inherited." The explanation appears to be as follows: Where a woman is absolutely sterile she of course leaves no issue and thus this characteristic cannot be directly "inherited." But in other cases, she may not be absolutely unprolific but only relatively so. Perhaps her first child is born with great difficulty and she is never able to have another. This may be due to structural conditions rendered worse by the first childbirth, or, her reproductive energy may become exhausted with the first child. This last is a phenomenon not infrequently seen, but very difficult to explain. For example it often happens that a couple have no children for a number of years after their marriage. Then the condition of the wife which is at fault changes and children are born. To again illustrate by an historical case,—the marriage in 1615 of Louis XIII of France and Anne of Austria. This marriage was without issue for over twenty-three years, when Louis XIV was born. In other instances, a woman has a child early in married life, and then not again until many years later another is born much to the surprise of everyone, including the woman herself. Almost everyone can cite cases such as these from his own observa-

[2]MacLaurin, A. "Mere Mortals."

tion. The explanation seems to be, not voluntary regulation, but that the woman's reproductive powers, exhausted by the first birth, have made a recovery during the long interval.

Such a tendency, inherited by the daughter from the mother, offers, I believe the most satisfactory theory by which to explain the relative sterility of the "heiress." That the tendency to have a large or small number of offspring is hereditary has been proven by experiments with domesticated animals, as Galton has indicated. By carefully selecting the most prolific individuals, breeders have developed stocks of animals much more prolific than the same animals in the wild state, or their domesticated ancestors of a hundred years ago. This is especially true in the cases of the domesticated pig and chicken, but is probably true of all animals raised for food, and for cultivated fruits and vegetables as well.

Having thus discussed the theoretical aspects of the question under analysis, we may proceed to the examination of the data upon which Galton based his hypothesis. This is given in the eighth chapter of his "Hereditary Genius" dealing with the causes of the extinction of the English peerages. Galton there lists twenty-nine cases of heiress-marriages. Of these, in eleven instances the marriage was without issue, while in eight only one child was born. In no instance did the number of children exceed four. Thus the percentage of sterile marriages was 37.9, and the percentage of marriages issueless or resulting in not more than one child was 65.5.

It is interesting to compare the fertility of the heiress marriages with that of the population as a whole. Galton states that of the wives of peers, one hundred who were heiresses averaged four children, whereas one hundred who were not heiresses averaged six children. Unfortunately he gives only these totals and does not give any of the details so that this data cannot be reanalyzed. We may, however, compare the frequency of the occurrence of sterility in the twenty-nine heiresses and in the average woman. According to Huhner, in the present American population 10 per cent of marriages are issueless. Popenoe and Johnson[3] give a figure of 8 1-3 per cent.

Accordingly, then, the "heiress" is approximately four times more likely to be issueless than is the "average woman." Or, to cite our data in another way, if a man marries an "average woman" the probabilities of issue are 90 to 92 per cent, whereas if he marries an "heiress" this probability drops to 62.3 per cent and the probability of there being more than one child is only 34.5 per cent.

Although the number of cases cited by Galton is perhaps too small to establish his conclusion beyond a reasonable doubt, the writer believes it is sufficient to indicate that a tendency of "heiresses" to be less prolific than the woman who has brothers and sisters really exists, and that a more extensive investigation would bear out his theory. As the question is a most important one, it is desirable to test it fully by collecting data on a large number of cases. This the writer has been doing for several years, but as a sufficient number of cases has not yet been acquired, and as many of the subjects have not yet passed the child-bearing period, these data will not be considered in the present discussion.

[3]Popenoe and Johnson. "Applied Eugenics."

The Mental Defective

CLINICAL PSYCHOLOGY *by* LOUIS E. BISCH. 346 pp., price $3.00, Williams and Wilkins Co., Baltimore, 1925.

Dr. Bisch writes as a physician and an intelligence examiner, with the object of supplying a text book and practical guide for teachers and doctors to enable them to recognize, classify, and handle mentally deficient or atypical children. Many chapters will be of especial value to lawyers, juvenile court judges, social workers, school nurses, etc. After a brief historical sketch of the treatment of mental defectives, outlines are given for taking detailed records of individuals, and in the appendix are presented over one hundred pages of actual case histories.

A discussion of the normal and precocious child in chapters IV and V is followed by chapters on the mentally retarded, the mentally arrested, the congenital mental defective, the epileptic, the syphilitic, etc. The author uses the word "inherited" (p 156) in speaking of a contagious protozoon disease when the disease is contracted before birth, a use which should be avoided with greatest care to prevent confusion. On the same page, to explain the infection of an embryo or baby, when the mother is unaffected, the statement is made that the ovum is infected by a protozoon. This seems too theoretical and hardly warranted by what is known of human embryology.

A good but very brief discussion of hereditary mental deficiency is included and its prepondering influence as a cause appreciated. A quotation is given (p. 168) from Drs. Shuttleworth and Potts' book "Mentally Deficient Children." In part, it reads: "If both parents are defective, every one of the children will be defective." Though this is probably true in most cases, because both parents are likely to carry the same gene for defectiveness (coming as they usually do, from the same community or from the same family), it is not necessarily so, for the defect may be due to different complementary genes and the children may be normal. The same statement is commonly found in books on genetics, but Goddard and others have reviewed cases in which the children were normal though both parents were defective*. The less evident defectives are by far the most numerous and probably inbreed relatively less frequently than those of the lower grades of mentality. If the defect were carried in different genes in different stocks of the population, children resulting from the mating of two relatively high grade defectives might be normal, and such cases may be common.

To the reviewer the discussion of the inheritance of mental defects might well have been greatly extended and is certainly needed to preserve a fair balance in the mind of the reader. A brief discussion of the inheritance of physical defects, of the frequency of the various types of defectives, the effect of their elimination on succeeding generations, the possibility of recognizing heterozygotes carrying defective mentality in their germ plasm, and the importance of family pedigrees might well have been included.

On the whole, the book should prove very useful. The cost of defectives is already a burden to civilization. The kind of information given here is necessary to make it possible to recognize the various groups and to treat and segregate them as conditions require. The spread of such information cannot come too quickly.

G. M. Darrow.

*See also HUNT, H. R. Intelligence as a Mendelian Character. *Journal of Heredity*, 17: 53-58. February, 1925.

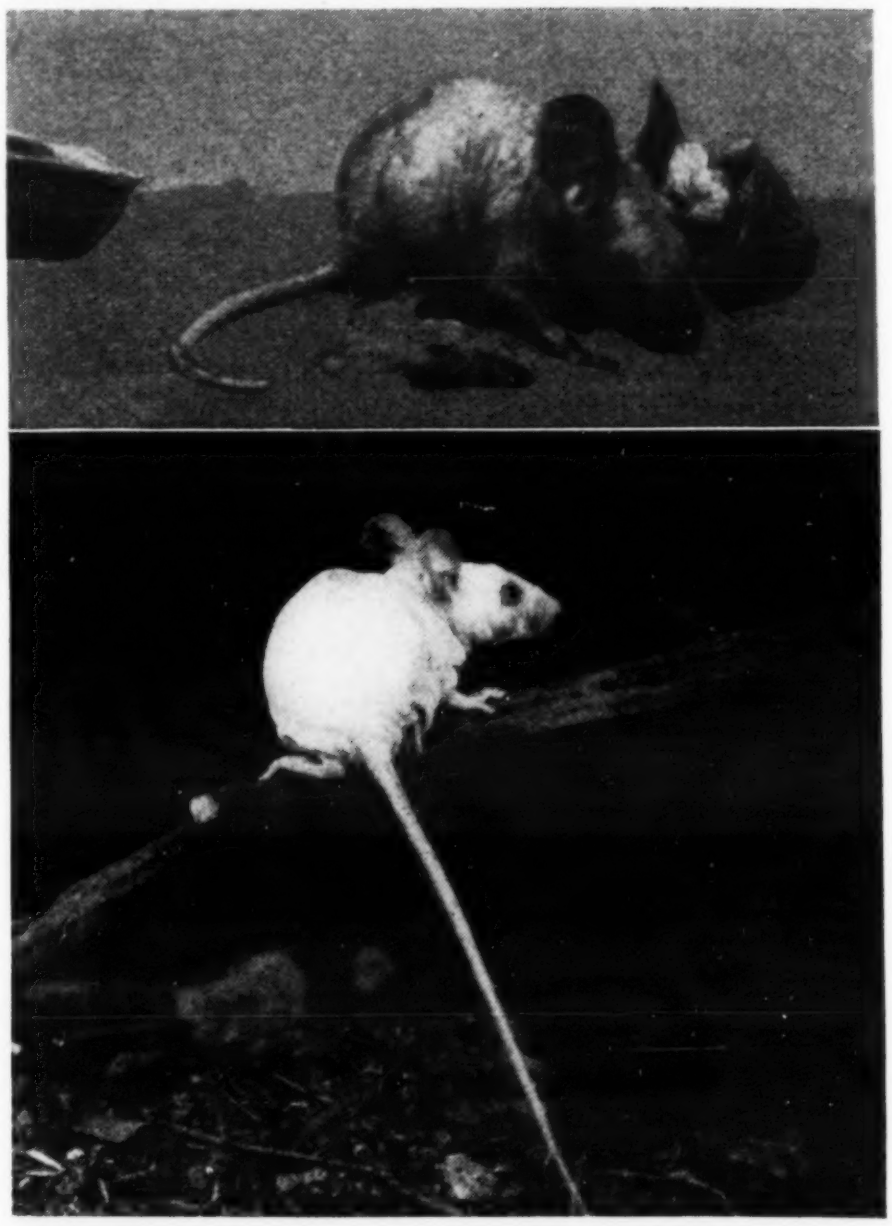
HAIRLESS MICE

H. C. Brooke

IN November, 1924, I received from a gentleman in North London, a pair of pink, smooth-skinned Hairless Mice, which he had captured in his aviary. During the previous half year, he had noticed the existence of other individuals, one of which was found lying dead, and another having been killed by his terrier dog. My observations on these animals differing somewhat from those made by Dr. Sumner and recorded in the JOURNAL OF HEREDITY,* and also from those made in earlier years by Gaskoin and by Campbell, I now take the opportunity to add a few further facts to our knowledge of this curious mutation.

These mice, of course, belong to *Mus musculus*, and it is a great pity that these are not fertile with *Peromyscus*, for, as Dr. Sumner observes it would be of very great interest to cross two strains behaving differently, and note the results. As, however, these Hairless Mice only crop from time to time, we are faced with the difficulty of obtaining two contemporaneous strains with which to experiment. Owing to the accidental mixing of young mice whilst I was worried and engaged with a great family trouble, I do not intend to enter upon accounts of proportions obtained which might not be strictly accurate.

My present strain shows a marked contrast to a Hairless Mouse I obtained three years ago† which cropped up unexpectedly in the stock of a breeder of Fancy Mice. This mouse showed every symptom of its condition being pathological; it was the color of a hippopotamus, sluggish, weak-eyed, and the skin was scurfy (Figure 4). The specimens I now have are most remarkably active, healthy, bright-eyed and well-shaped animals, quick as lightning in their movements. The specimens described by Gaskoin and Campbell (1856, 1907) all seem to have shown pathological symptoms and to have become decrepit at an early period of their existence. The single mouse above referred to (Figure 4), when about six weeks old, grew a short coat, looking as if it had been singed, which disappeared



TWO TYPES OF HAIRLESS MICE

Figure 4

The upper picture shows a hairless mouse that appeared in the stock of a breeder of fancy mice. It is sickly and weak-eyed, in contrast to the mouse shown below, which is one of the original pair of wild hairless mice described in this article. This strain is active and healthy, and does not show the pathological characters usually associated with hairlessness in mice.

*Volume 15, No. 12, December, 1924.

†This is the mouse referred to by Dr. Sumner as described in the Amateur Menagerie Club's Yearbook.

in a few days. Nothing of the kind has been observed in these mice; but a very similar phenomenon was noted years ago in the case of New Mexican Hairless Cats—concerning which I should be grateful for any information. Up to now these mice have not acted at all "according to plan" in their reproduction. From the Hairless male × Hairless female were produced all normal young; as also from the Hairless male × a black and white "Fancy" female and by an albino female. From these, too, I only obtained one male in four litters, all the rest being females. These young females I mated back to their sire; but owing to the unfortunate mixing above referred to I can not give exact data as to proportions. From several litters I obtained nine male hairless and no females. Then suddenly females appeared and I now have several of them. Altogether I now have seventeen Hairless against about eighty normal young.

The depilation process takes the same course in all specimens. Commencing about the fifteenth day of life, the fur first comes off on the bridge of the nose; in two days the head is bare to the ears; the fur then disappears regularly with increasing rapidity until by the eighth day of change the mouse is entirely devoid of fur. *Vibrissae*, of course, are always present. At first, the skin of the young Hairless Mice is a bright shrimp pink; as they grow older the skin turns paler, until in the adults it is almost a dead white.

The original specimens must now, at the time of writing, be just a year old, but seem quite well and hearty and show but little diminution of vitality. Whilst their skin is certainly more wrinkled than that of the young animals, they in no way approach a "rhinoceros" phase.

As regards fertility, I have not yet been able to make any observations concerning the mating of young Hairless male with young Hairless females (the Hairless female specimens not yet being old enough), but in every case when I have placed a young Hairless male with a furred heterozygous female he has proved himself a stock getter. The furred young in the same litter with Hairless young are sometimes normal wild color, sometimes blackish brown. The young obtained by mating a Hairless male with an albino female (albino-bred for generations, I understand) also answer the above description. These are not yet old enough to breed.

One particular furred female, by the original Hairless male × a black and white female, has now produced four litters sired by a Hairless male. The proportions have varied, her last litter consisted of two furred, one hairless; the previous litter was two hairless, two furred. But in every case hairless young have been present.

Figure 4 shows a young Hairless Mouse from the original wild Hairless pair (photos by Miss Frances Pitt, Bridgnorth).

About three years ago I received from a London Fancy Mouse breeder, three young pink Hairless Mice, which however, only presented an instance of delayed hair development, as at the ages of about three, four, and five weeks, respectively, they grew fur in the normal manner, and remained normal. I did not obtain any young from these specimens.

NOTE:—Since the above was written some months ago, I have been able to observe that some of these mice, when old, do become very wrinkled and exhibit deformed and twisted claws on the hind feet. In no case, however, has the wrinkling of the skin in any way approached that recorded by Campbell, for neither activity or sight has been affected. I have also obtained a few albinos. Not a single female (except the original mentioned above) produced young; they all seem to be sterile and some of the males also. In the furry halfbred specimens the females very greatly outnumber the males. Continued illness has prevented my making further observations.

—*H. C. B.*

BIOLOGICAL CONSEQUENCES OF RACE-CROSSING

Jon Alfred Mjoen

PROFESSOR W. E. Castle, who is well known over here for his successful experiments with animals, has in the Journal of Heredity for September, 1924, come forward with some critical remarks on my views respecting Race-Crossing.[1] Professor Castle's article is in the nature of a reply to an article by Professor Stuart A. Rice.[2]

Professor Rice's standpoint towards the question as to whether *biological forces* affect the development of human society is indicated by himself as follows: "Enough has been said of this question to show that a biological law of the kind suggested by Dr. Mjöen's studies *may possibly* be setting a lower limit to the possibilities of racial and cultural amalgamation between two peoples so diverse as the two major races in the United States."

Professor W. E. Castle puts forward his views in the following statement: "Human race problems are not biological problems any more than rabbit crosses are social problems. The sociologist who is satisfied with human society as now constituted may reasonably decry race-crossing. But let him do so on social grounds only. He will wait in vain, if he waits to see mixed races vanish from any biological unfiitness."

Professor Castle's article is mainly formed as an attack on my studies regarding Harmonic and Disharmonic Race-crossings, and it offers me a not unwelcome opportunity of coming forward with some supplementary remarks to my studies.

Professor Castle asserts that "most inherited characters are blending. When parents differ in a trait, the offspring commonly possess an intermediate degree of it. This is true of stature, weight and, I think, of general mental powers."

In a crossing between individuals of different races (i. e., individuals whose idiotypica (genotypes) diverge from each other beyond a certain degree of variation*) we may imagine four possibilities as to the combination in which the (hereditary) qualities will occur in the next and succeeding generations:

I. The offspring inherits all essential characteristics from *one of the two races*. (Prepotency of one race over the other.)

II. The offspring inherits from both races, but inherits all characteristics, from the color of the eyes to the highest mental functions, as a homogeneous *blend* of the somatic and psychic constitution of both parents or both parent-races.

III. The offspring inherits certain characteristics from each of the two races. The large lungs, for instance, from the Nordic race, the small heart from the Lappish. "Size and function are to a certain extent independent of each other."

IV. The offspring inherits each characteristic from both races, as in a kind of partialism where every particle, every organ becomes a mosaic composed of heterogeneous hereditary qualities.

Amongst the mixed population in

*This definition corresponds to Dr. H. F. K. Günther's definition of the term "race": "A race represents a group of human beings which through its peculiar combination of physical traits and psychic qualities differs from every other group and always produces only its like." (Rassenkunde des deutschen Volkes.)

North Norway I have found blue-eyed Lapps and Lapps with dolichocephalic heads, blond hair and reddish hair, narrow nose, straight nose, light Nordic complexion, etc., all being characteristics which have arisen from intermixture with the Nordic population, and which occur again in later generations. The blue eyes are rare, but in some cases they are just as blue as those of the Nordic ancestors that have mixed their blood with the Lapps. The discovery of such Nordic features or qualities amongst the Lappish population, in individuals that besides bear unmistakable Lappish characteristics, constitutes so many proofs of the fact that it is not the one race alone that imprints its stamp upon the hybrid offspring (1). Further, the facts observed furnish proof that the qualities of the two ancestral races are not mingled so as to form a homogeneous blend of the parents' or parent-races' characteristics. If this were the case, then blue and black eyes, for example, would necessarily disappear by degrees from amongst the Lappish-Nordic hybrid population, never to appear again, whilst a mixed intermediate shade would have become the only prevailing color of the eyes of the hybrids in North Norway.

The actual facts seem to speak a different language than Professor Castle, seeing that they furnish unequivocal proofs that the majority of human characteristics do not blend through race-crossing extending over one or several generations. It even seems to me probable that no hybrid qualities in the deepest meaning of the expression (bio-chemical and cell-anatomical) form a homogeneous blend of the traits of the parent-races, although on superficial examination this may appear to be the case.

One of the strongest proofs of the assumption that the offspring receives a homogeneous blend of the parents' qualities was thought to have been found in measurements of the Head Index.[4]

As regards the Head Index, so far as I can see from the latest literature, H. Bryn, E. Fischer, Davenport, Frets, Hauschild and Kaarlo Hildén, have abandoned the idea of a blend if they ever had it. In this connection it is only necessary to cite Docent Robert Larsson, Lund: "Davenport's compre-

TABLE I—Inheritance of musical ability in which the offspring are intermediate between the parents. (The degree expressed in rank from 1-10.)

Parents:	*Offspring:*
9 × 2	5, 6.
10 × 4	5, 5, 6, 4.
4 × 1	2, 3.
1 × 6	3, 3, 5, 5.
8 × 5	7, 7.
0 × 5	2, 4, 3.
3 × 0	2, 2, 2.

TABLE II—Inheritance of musical ability: Offspring not intermediate

Parents:	*Offspring:*
5 × 4 =	6, 7, 0, 2.
5 × 4 =	6, 6, 7, 7, 8, 6.
7 × 5 =	8, 9, 7, 8, 5, 5.
4 × 5 =	7, 7, 5, 5.
4, 5 × 4 =	5, 5, 5, 5.
6 × 5 =	5½, 7, 5, 9, 10, 8, 9.
6 × 5 =	3, 4, 5, 3.
4 × 5 =	7, 5, 5.

TABLE III—Musical ability in racial stocks, parents and offspring

Average of the "Side-lines (Stocks)"	*Parents:*	*Offspring:*
6 × 7	6 × 4	6, 8, 7, 6, 5.
3 × 5	6 × 5	3, 5, 3, 3.
7.5 × 6.5	5 × 3	5, 6, 5, 6, 6.
3 × 2	5 × 6	4, 3, 2, 3, 3, 3.
7 × 5	5 × 5	8, 7, 6, 6, 7.
6 × 6.5	9 × 8	7, 6, 7, 8, 7.

hensive investigations regarding crossings between white and colored persons must surely at last have banished from the world the old views respecting intermediary inheritance amongst human beings." Eugen Fischer writes: "In the case of the human being, exactly in the same manner as has been shown by thousands of experiments on animals and plants, the separate characteristics are inherited either as dominant or recessive."[8]

From a superficial observation it might seem as if the qualities blend when the characteristics of the offspring lie between those of the parents.

Intermediary figures which might be interpreted as denoting a "blend" we find also in our measurements of musical ability[7]:

At the first glance it would seem from this as if the child represents an intermediate stage and it would give Professor Castle an apparent foundation for his assertion that "human traits blend." But we have begun to doubt whether the so-called intermediary figures can be interpreted as a proof that the parents' qualities blend. As regards the musical facuity we find in the first place many divergencies from the intermediary figures. (See Table II.)

For a better understanding of inheritance in man, it is well to remember that it is the congenial stocks rather than the congenial parents who determine the extent of ability. When the offspring vary widely from the parents, the explanation is to be found in the side-lines (See Table III).

There are now only two possibilities left (compare III and IV). *Whether we assume one of these possibilities or both, there is an unfailing certainty that all conditions are present for a large number of anatomical and biochemical disharmonies.*

I speak of a disharmonic crossing in the narrow sense when the offspring's adaptability and efficiency show a decrease in comparison with each of the parental stocks, as a result of special combinations of hereditary qualities in the offspring (A) and in a wider sense I have used the term "disharmonic" crossings for such as are unfavorable even if compared only with one of the two races (B).

By adaptability (a word to which special weight is attached in the above definition) I mean the term taken in its widest signification: (1) Every organ's special capacity for co-operation, (2) the whole organism's power of coping with the demands made upon it in the environment in which the individual is placed, (3) the individual's power of accommodating himself to the human fellowship and of adapting to each other his various conflicting impulses and instincts, so that no fatal obliquities will arise in his morals or mind.

Let me mention some examples of how this general adaptability may be hindered in various ways by disharmonic crossing.

A. The offspring inherits from one of the parent races (for example, the Nordic) large lungs (large in comparison with those of the other race), from the other (for example, the Mongoloid) a small heart (Ploetz).

B. A healthy man of Nordic race marries a healthy Lappish girl. He expects of his son, as of himself, that he shall take part in the work on the farm he owns. But the son proves to be incapable of hard work. The feeble muscles, with little capacity for development, inherited from the mother's race here form an unfavorable combination with the father's social position and ambition.

I am afraid that I dare not take up more space in this journal by citing more detailed instances. Let me merely remark that the psycho-technical measurements that have been carried out during a long series of years at Winderen Laboratorium of individuals of Nordic and mixed (Nordic and Lapp) population in con-

nection with the somatic investigations that have been made at my laboratory and elsewhere, *have yielded innumerable proofs of the biological importance of the problem of race-crossing.*

Even the pigmentation of the skin, which is generally considered to be of very little importance for the race, *might*, according to my opinion, give rise to disturbances in the hybrid. The organs of the body are possibly sensitized for certain colors of light To say that it is of no consequence or significance that organs or parts thereof by race-crossing become clothed with a skin of quite different quality (color) is to take a responsibility we are not justified in taking by our present knowledge. I should be glad on another occasion to refute Professor Castle's amazing assertion that: "So far as biological considerations are concerned there is no race-problem in the United States." This assertion contrasts strangely with the statements made by another eminent representative of American science, Professor Henry Fairfield Osborn, during his visit to Norway's capital in 1921.

Now to turn to the animal experiments. Professor Castle can certainly not be contradicted when he writes that it is necessary to be very cautious in drawing conclusions. He mentions in this connection the importance of feeding. I know that food has a marked influence upon the sexual instinct in animals and probably also in humans in the long run upon fecundity. I admit that my F_5 and F_6 hybrids would possibly have been more lively in mating and would have shown greater fecundity, *if I had fed them extra well.* But just the circumstance that this group of animals, under the same conditions as all the other groups of unmixed races and their own ancestors, show decreasing vitality and fecundity, as compared with the pure races, renders in my opinion the result of the experiment applicable as a generalization. Of course it can only be applied for the purpose of setting up a problem, the solution of which can only be obtained through an abundance of anthropo-biological observations. But the facts observed in the case of the rabbits yields material for a cautious anticipation of the conditions respecting fecundity amongst human hybrids.

I am willing to admit the possibility that there are to be found *pure* races of rabbits with the characteristic: one upright and one drooping ear. I have not seen such. Neither did my contention amount to a denial of this, but rather to the assertion of the fact that the crossing between the races I have chosen will produce a not inconsiderable percentage of animals showing this assymmetry in ear-carriage. And this striking assymmetry ought to make us suspicions as to other less visible traits.

Let us repeat the principal analogies:

1. *Human hybrids*: distorted features, long faces, relatively large or small ears, disproportionate extremities, etc.

Rabbit hybrids: frequently one standing and one hanging ear, etc.

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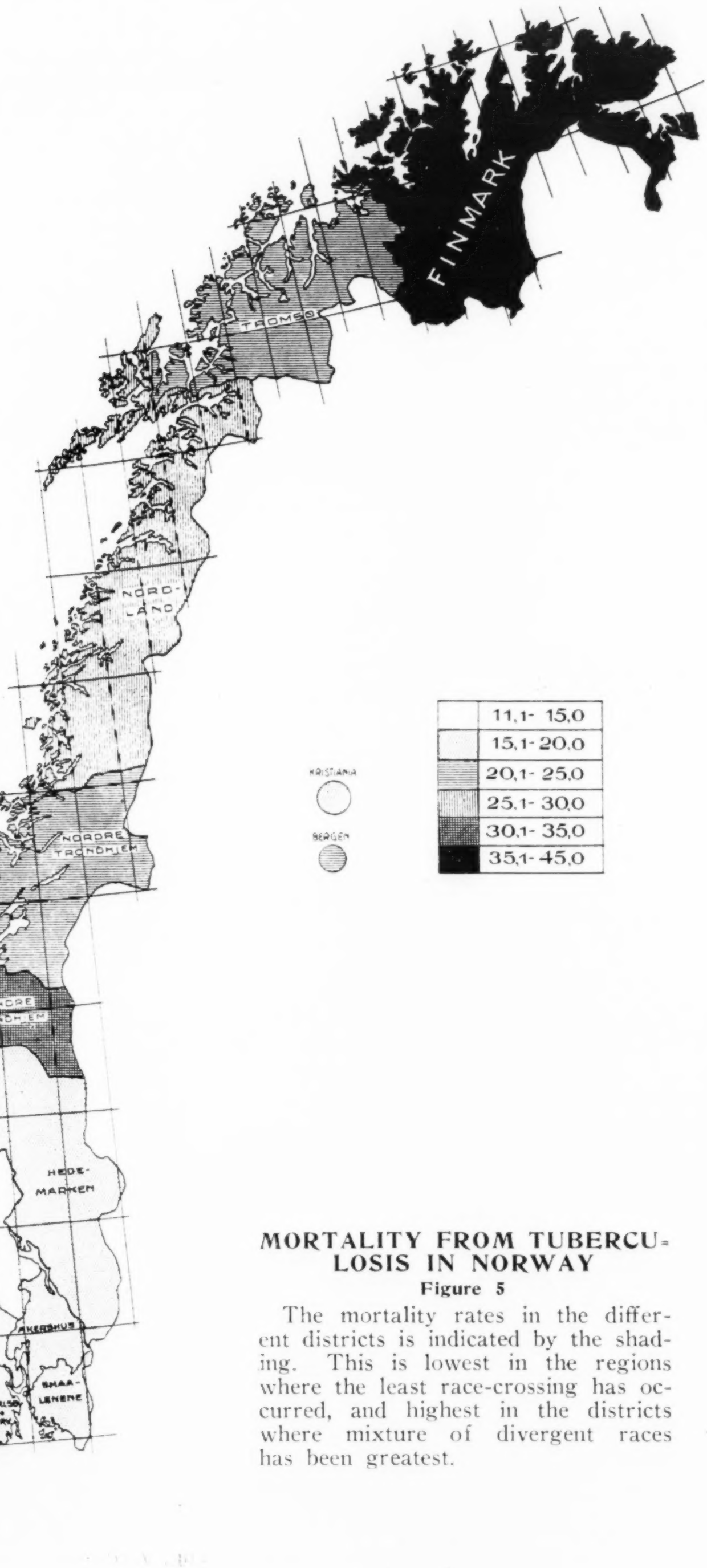

MORTALITY FROM TUBERCULOSIS IN NORWAY

Figure 5

The mortality rates in the different districts is indicated by the shading. This is lowest in the regions where the least race-crossing has occurred, and highest in the districts where mixture of divergent races has been greatest.

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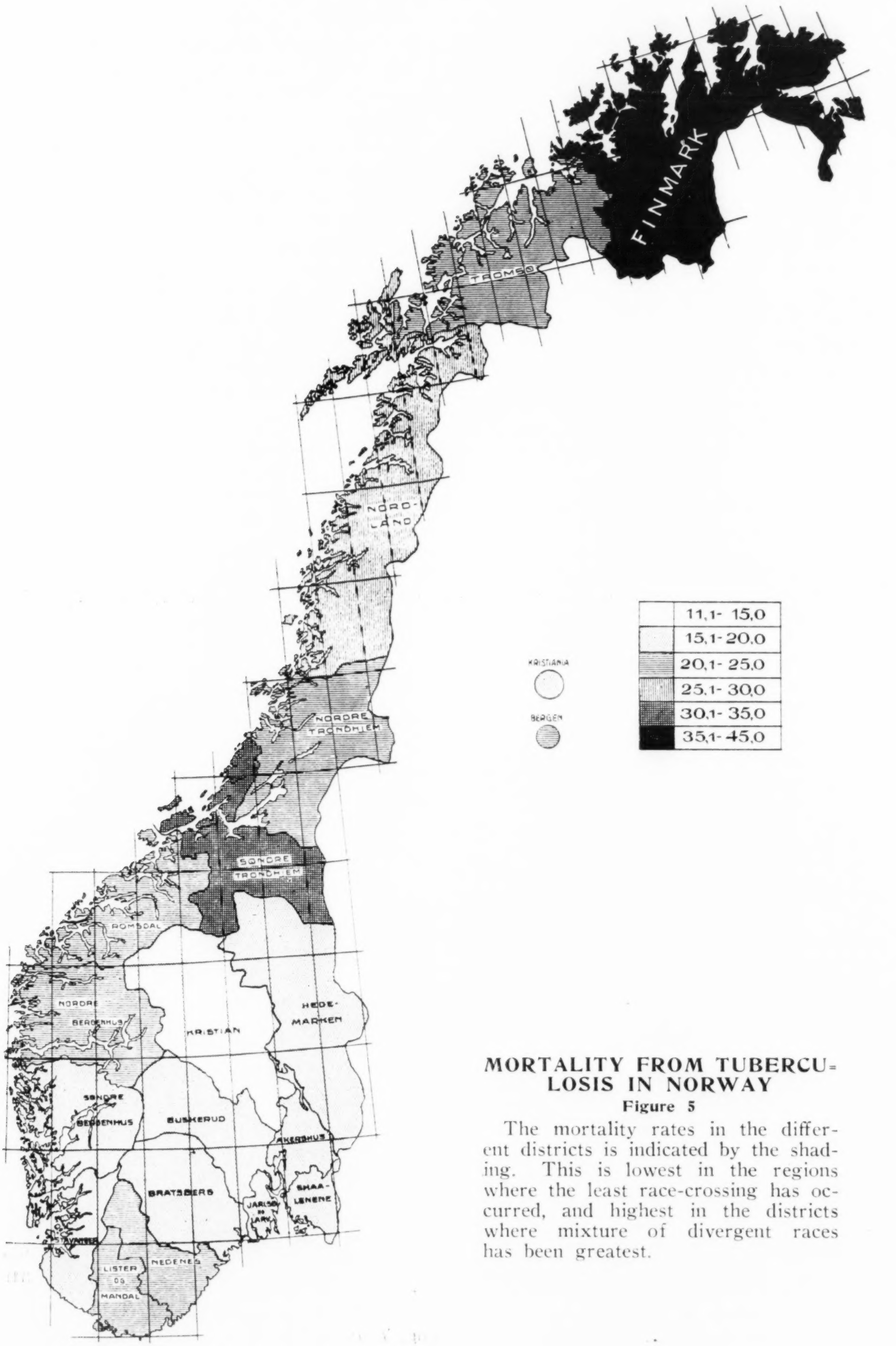
These analogies between human and rabbit hybrids can be considered established.

Professor Castle gives an entirely erroneous presentation of my conclusions, when he states that from experiments on animals I draw conclusions regarding the human material. I *had* already made my observations on



MORTALITY FROM TUBERCU=LOSIS IN NORWAY

Figure 5

The mortality rates in the different districts is indicated by the shading. This is lowest in the regions where the least race-crossing has occurred, and highest in the districts where mixture of divergent races has been greatest.

human crossings and drawn my hypothetical conclusions from humans, when I commenced experiments on animals in order to inestigate (1) whether in crossings of dissimilar races there occurred signs of disharmonies in the offspring, (2) some phenomena respecting heterosis, and (3) the hybrid's fecundity and power of resistance against alcohol, immunity against tuberculosis and disharmonies in size of organs. These latter experiments were not carried to an end, but I am sure I could trace a decrease in fecundity, as stated.

The hypothesis drawn from observations of humans is not weakened but strengthened by the fact that it finds support in analogous observations in another field of investigation, namely, animal experiments, especially when we remember the universalism that seems to characterize the laws of heredity.

So far from drawing conclusions from experiments on animals *alone*, on the contrary I uttered a warning against such conclusions, *nay, I have even warned against drawing conclusions from one human crossing (races) to another.**

As stated above, it is not my opinion that hybridization will "set a lower limit" for the racial and cultural amalgamation of two different races in a community *only because the hybrids become less fertile*, but also because their physical and psychic health, their adaptability and their immunity are reduced as a result of frequent disharmonies in the combination of hereditary qualities.

The map of Norway, reproduced on the preceding page illustrates that the mortality from tuberculosis is largest (and very large) where there is a considerable mixture of races. My observations among mixed populations, published first in 1908 and later in a book on Race Hygiene (1915) conclude that there is a causal relation between race-mixture and diminished immunity.

In addition to a disposition to tuberculosis, race-crossing seems also to be a contributory cause of alcoholism, criminality, infantile mortality and psychic disturbances in the mixed population. I find my conclusions supported by Dr. Halfdan Bryn and by Professor Lundborg.[5] Lundborg writes: "Children begotten by persons belonging to entirely different races (which races may each in themselves be of high quality) have great possibilities of acquiring unusual gene-combinations. The constitution does not maintain its vigor, which fact is revealed in lack of adaptability, *e. g.*, increased disposition for illnesses (tuberculosis, etc.) weakness of character, criminal tendencies and other deficiencies."

It has been ascertained from Lundborg's investigations that a large town, Svappavaare, which was colonized in the seventeenth century by Swedes from Värmland and Dalsland, now has a population greatly mixed with Finnish and Lappish elements. At the present time tuberculosis in many forms is very prevalent there. The sanitary conditions, dwellings and other factors of environment are by no means worse in Svappavaare than in other towns, but, on the contrary, rather better. The environment theory, therefore, cannot explain this fact. We are forced to assume a difference in disposition. The Swedish-Finns in Svappavaare are of more mixed race than the inhabitants of other towns in the same district, and herein, no doubt, lies the reason. On the coast of Labrador there is now living a mixed population, which sprung up during the first half of the nineteenth century, through a crossing between English and Scotch hunters and Eskimos. The parent stocks, both Europeans and Eskimos, were healthy and

*(See point 9 in my Summary in Paper read at Second Eugenics Congress, New York, 1921: Harmonic and Disharmonic Race-Crossing, *Eugenics in Race and State*, volume II.)

reached an advanced age, but the offspring have proved to be feeble and suffer much from consumption. (Zollschan).[5]

I have found it to be in harmony with human interest to maintain that, *until we have acquired sufficient knowledge of these difficult biological questions, there is reason for caution with regard to race-crossing between two so widely divergent races as the Nordic race and the Mongoloid Lapps.*

When I in 1908 drew attention to the fact that there is a causal connection between mortality from tuberculosis and race-crossing, I was not acquainted with any of the works which I here quote. These works had not been published at that time. I was uninfluenced by the opinions of other investigators.

Professor Castle scoffs at the terms "harmonic and disharmonic race-crossing," which have been introduced by me, and tries to explain them as having arisen in a brain biased by racial pride and racial prejudices. If my feelings on this point is of any interest whatever (feelings have generally very little to do with science) I should like to express it in the following words: I do not propose to prevent race-crossings on the ground that we are so much better than the other races. We shall love and protect each of us our own race for the same reason that we love our father and mother: Because it is our race!

It is not contempt and arrogance towards an "inferior" race that leads me to adopt a hostile attitude towards the above-mentioned crossing. It is my conviction that it is to the benefit both of the individual and of the two races that such a mixture of races does not take place, at any rate so long as we do not know how we can reduce the number of unfavorable and unfortunate combinations of hereditary dispositions. My observations have shown me that the possibility of such unfortunate combinations is very great.

Summary

The present article aims at showing that the problem of race-crossing is just as much a biological as a social problem, nay, more, that it in reality acquires a social interest first and foremost in virtue of its biological character.

The majority of human characteristics do not blend. It is the individual traits that are inherited, dominant or recessive. Thereby the possibility is offered for a number of so-called disharmonic race-crossings.

I speak of a disharmonic race-crossing in cases where the adaptability and efficiency of the offspring shows a decrease in comparison with the parent races, as a result of special combinations of inherited qualities in the offspring.

The probability for unfavorable combinations in the offspring seems to increase proportionally with the relative dissimilarity of the parental idiotypes (genotypes), and should therefore attain its maximum in case of the crossing of races which in biological respects are farthest removed from each other.

Until we know more about these difficult problems there is ground for caution with respect to crossing between races that are remote from each other in biological respects. My observations point to the *considerable probability* of an unfortunate result—a disharmonic crossing: reduction of lung capacity, weakening of muscular power, decrease of power of resistance against disease, increasing infantile mortality.

The problem as to whether *fecundity* decreases as we come far down the hybrid generations does not appear to be finally solved by science in its present stage.

I am well aware that crossings *within the limits of one and the same race* may also produce disharmonic combinations of hereditary characteristics. *But I venture to postulate that*

the probability for unfavorable combinations in the offspring increases proportionally with the relative dissimilarity of the parental idiotypes, and should therefore reach its maximum in crossings between races which from a biological point of view are farthest removed from each other.

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2. Biological Limits in the Development of Society. *Journal of Heredity*, 15:183-185. April, 1924.

3. Bauer, Fisher, E., Lenz,

4. See Hilden, Kaarlo., *Hereditas* I, 1925. The work of H. Bryn is cited and the head indices of parents and offspring are given. The F_1 indices nearly all are intermediate between those of the parents.

5. Kultur och Rasproblem i Medicinsk-Biologisk Belysning. "Tuberculous disposition and genotypical degeneration regarded from the standpoint of medicine and theory of heredity," published in Stockholm in 1920.

6. Compare also Baur, Fischer, Lenz, H. Günther, Ch. B. Davenport, Goddard, Stoddard, Oswald Spengler and, as far as I understand, also Professor Rice in Journal of *Heredity*, April, 1924.

7. Mjöen, Jon Alfred, zur Erbanalyse der Musikalischen Begabung, *Hereditas*, 1925, Bd. VII, 1.

Comment on Dr. Mjoen's Paper

By W. E. Castle

THE Editor of The Journal of Heredity has invited me to comment briefly on Dr. Mjöen's paper, which I am glad to do, in order to clarify if possible the reasons for our divergent views. Mjöen misapprehends my meaning in his adverse criticism of the expression "blending inheritance." I mean inheritance which is phenotypically blending but which in all recent text-books of genetics (including my own, *Genetics and Eugenics*, 3d Edit., 1924) is interpreted as *multiple factor* Mendelian inheritance. Whether or not musical ability is inherited in this way, it is difficult to judge because it is well nigh impossible to measure the character accurately, as could only be done by completely dissociating inborn qualities from the effects of training. The case illustrates the fundamental difficulty in analyzing the inheritance of human characteristics other than purely morphological ones.

When it comes to the size of particular organs, for example hearts and lungs, we are on safer ground, but even here there are practical difficulties. Who has measured accurately the size of the heart (constantly changing in volume) or the lungs (collapsible sacs) in three successive generations of human beings? It might be better in measuring organs to begin with the size of hands or feet, but here we should need to consider the mode of life of the individual, whether he worked with his hands, whether he wore shoes. To assume that a farmer's son has inherited weak muscles because he is unwilling or unable to do farm work is rather jumping at conclusions, is it not? Such disinclination or disability is often found in pure Nordic families where no disharmonic race crosses are involved. I mention these matters merely to show the extreme difficulty of acquiring data concerning human inheritance and the wholly uncritical character of Mjöen's material.

Mjöen recognizes four (and only four) *possibilities* as regards the inheritance of characteristics in race crosses. No one of these conforms with an *actuality* described in any work on genetics with which I am acquaint-

ed. The idea which Mjöen seems himself to favor is that organs as such (or their genes) are inherited, that an individual may inherit one organ from one ancestor, a different organ from another ancester. He mentions specifically Nordic lungs, Lapp heart; Nordic skeleton, Lapp muscles. This seems to be the underlying idea of his belief in "disharmonic race crosses." Now it will suffice to say that neither in the work of Mendel nor in that of any of his followers (among whom I am happy to count myself), has there been demonstrated a single case of inheritance of organs as such. Organs are not unit characters. The blue Nordic eye is classed as a unit-character recessive to dark eye, but this must not be understood to mean that the eye as found in a Nordic individual will be inherited as a structural or a genetic unit among his descendants. The size and shape of the eye for example have nothing to do with its color, being determined by independent genes. Not only is the eye as a whole not a genetic unit, neither is one of its parts, as the retina, the choroid, the iris, or the eye-muscles. The "blue Nordic eye" means nothing more structurally than absence of black pigment from the front wall of the iris in an otherwise normally pigmented eye. Genetically it is a peculiar state (mutation) of a particular one of numerous genes necessary for the development of pigment in the eye. Mjöen's apprehensions (they are nothing more than that) concerning the possible evil biological consequences of race crossing rest entirely on a misconception of the fundamental law of heredity (Mendel's law). His human evidence is, in my opinion, no better than the rabbit evidence, which I showed in a former paper to be uncritical.

Dr. Mjoen's Reply

I REGRET that I have understood Professor Castle's term "blending inheritance" as an expression for the earlier assumption that some qualities do not follow the Mendelian heredity laws, but that the traits of the hybrids rest on a blending of the hereditary factors from the two parents and manifest themselves as intermediate between the (allelomorphic) parental qualities, an assumption for which, it seems, Professor Castle has been the most ardent advocate. If, for instance, we open Babcock and Clausen's *Genetics in Relation to Agriculture* (1916), we find under "Blending Inheritance recognized by Castle" p. 189, the following sentence: "On the basis of these and other facts Castle argues that we must recognize three types of inheritance:

1. Typical Mendelian Inheritance.
2. Typical Blending Inheritance.
3. Partial Blending Inheritance.

The definition of "typical blending inheritance" (p. 189) is: "Factors of allelomorphic characters blend in the heterozygote to form factors of intermediate character. *The factors of the heterozygote are as uniform* (!) *as those of either parent.*" The reader will in view of the above quotation admit that my "misapprehension" of Castle's term "blending inheritance" is both pardonable and justified. For Castle has obviously not long ago (1924) changed his mind and realized his own misapprehension of certain conditions. It is difficult to understand why he is still using the term "blending inheritance" which is misleading. That he uses the term for *multiple factor Mendelian inheritance* is, according to my opinion, as fallacious as it would be to use the term solution for an emulsion.

Prof. Castle's remark about "the fundamental difficulty in analyzing the inheritance of human characteristics other than purely morphological ones"

is a prejudiced view among geneticists, who work mainly with meter and microscope. We already know a series of psychic qualities—basal component faculties of musical ability—which perhaps depend as little on surroundings and effects of training as do "the size of hands and feet." We are also able to measure and control qualities which are far more characteristic for the musical ability than size is for an organ.*

That a hybrid between a Lapp and a Norwegian gets a more feeble constitution than the parental races, is a symptom which cannot be explained away by saying that similar "disinclination and disability is often found in pure Nordic families." Neither can the rabbit-hybrids with one standing and one hanging ear be explained away by the assertion that similar asymmetries may exceptionally occur in rabbits of pure race. The circumstance that a rabbit with one standing and one hanging ear will not be granted a premium —be it ever so "pure-raced."† might be a proof that such a quality is not only atypical for the race but also an "inferior" trait in the struggle for prizes and therewith for existence.

Since I, according to Castle's conception, seem to believe that the organs are inherited as such, because I use terms as "Nordic lungs," "Lapp heart," etc., it is perhaps necessary to emphasize that my expressions of course are not to be understood literally, just as little as it has been the conception of Ploetz, when he first drew attention to the fact that it was possible to inherit the "big lungs" from the father's side and the "small heart" from the mother's, that the organs as such (or their genes) are inherited. My terms "Nordic" and "Lappish" are in the present case only meant to express the difference in size between the organs of the two races. It seems to be probable that these differences may depend upon the presence or absence of Mendelian factors.

I have asked, Dr. Halfdan Bryn, Trondhjem, the most prominent of living Norwegian anthropologists, to give a commentary to the present discussion: His answer is given below and as he has the advantage of a very large material‡ his conclusions must be given considerable weight. Dr. Bryn writes:

> I have a firm impression that crossing between Lapps and Norwegians is equally bad for both parties. I have once in a treatise drawn attention to the great number of cases of *luxation coxae congenitae* amongst the hybrid population in Finmark. It has repeatedly been demonstrated that this illness occurs with many times greater frequency amongst hybrids than amongst people of unmixed race. The explanation naturally suggests itself that this may be due to the fact that the offspring has inherited the small pelvis of the Lapps with narrow hip-joint socket (acetabulum) while at the same time inheriting the Norwegians' large thigh-bone head (caput femoris), which accordingly has not room enough in the small acetabulum. But this is of course only a suggestion. I have hitherto not been able to advance any proof of the correctness of the supposition.
>
> But that race-crossing often leads to disharmony is a point on which I entertain no doubt.

Finally, I shall once more repeat that my crossing experiments never pretended to have solved or definitely decided the problem. But together with the observations I have made for years among the hybrid-folk in North-Norway the experiments justify the conclusions that:

There are outward and inner signs to be found in animal and human hybrids to justify the characterization

*Compare JON ALFRED MJOEN: Zur Erbanalyse der musikalischen Begabung. *Hereditas* 1925, and also Hans Koch & Fridtjof Mjöen: Die Erblichkeit der Musikalität, *Zeitschr. f. Psychologie* Bd. 99, H. 122.

†According to a statement from the secretary of the "Norwegian Society for Rabbit-Breeding."

‡BRYN, HALFDAN: "Tröndelagens antropologi" (The anthropology of Trondelagen), 1920. "Antropologia Nidarosiensis," 1920. "Möre fylkes anthropologi," 1922. "Raceblandigger i Troms fylke" (Race-crossing in Troms). *Tidsskrift f. Den Norske Lægeforening*, 1922.

I have given certain crossings by calling them disharmonic.

We shall not encourage a feeling of animosity towards another race,—but all facts pointing in the same direction it is our duty to say: until we have more knowledge, we should avoid crossings between distant races. We shall love our own race not because our own race is so much better, but like father and mother, because it is our race.

Genetics for the Classroom

PRINCIPLES OF GENETICS, by EDMUND W. SINNOTT and L. C. DUNN. An elementary Text, with Problems. Pp. XVIII + 431. McGraw-Hill Book Co., New York. 1925.

Sinnott and Dunn's text is written consistently with the classroom in mind. The principles of genetics are developed simply and clearly. Each chapter is followed by well chosen lists of "questions for thought and discussion," "problems," and "reference assignments" calculated to make easy the work of the teacher if not that of the student. These questions, problems, etc., for the most part involve material not discussed elsewhere in the text and thus greatly extend the material covered by it.

The first two chapters introduce the problems of heredity and variation and deal briefly with their historical development. Chapters III to V cover the elementary principles of Mendelian heredity. The physical basis of Mendelian heredity in the behavior of the chromosomes is treated in chapters VI to VIII. Chapter IX deals with sex and its inheritance. Chapter X deals with quantitative characters and introduces briefly the biometric methods. Chapter XI discusses the types and causes of variation at considerable length. Chapter XII gives a well-balanced discussion of the application of present knowledge to plant and animal breeding. Chapters XIII and XIV treat of inheritance in man and the problems of eugenics respectively.

The treatment appears to the reviewer to be accurate, readable and otherwise thoroughly adequate for its purpose. It is a book which should come into very extensive use.

Sewall Wright.

Heredity and Milk Yield

MILK SECRETION, by JOHN W. GOWEN. 363 pages, price $4.50. The Williams and Wilkins Company, Baltimore, Md.

Dr. Gowen's book on Milk Secretion is a welcome contribution to our knowledge of the inheritance of milk yield and butter-fat percentage in dairy cattle. Publications on this aspect of dairying are relatively scarce and Dr. Gowen is to be congratulated for his achievement in producing a volume which is a pioneer in its field.

The present treatise really is a compilation of the author's many papers on the inheritance of dairy performance which have appeared in recent years. The subject matter is largely based on analyses of Holstein Friesian Advanced Registry records for milk yield and butter-fat percentage. The very nature of the various problems to be solved makes the extensive use of statistical methods imperative, and a considerable portion of the book is rather technical for the layman to understand. It appears that the author had two purposes in mind when he was composing the book, namely, to include enough of the methods of analysis and formulae

to make the volume a manual for research workers in this field and to include sufficient explanations to enable the dairyman and breeder to obtain necessary information without having to go into the methods by which such conclusions were reached.

The first six chapters deal with such subjects as the characteristics of Holstein Friesian milk yield; the relation of conformation to 7-day milk yield and butter-fat percentage; mode of secreting milk and milk solids; influence of age of cow; and the permanence of a cow's performance. Those who are interested in the judging of dairy cattle will find especially interesting the chapters on the relation of conformation of the dairy cow to her 7-day milk yield.

The 13 chapters which are devoted to the analysis of the relation of an ancestor's milk and butter-fat records to future performance disclose some very valuable information. First, the relations existing between butter-fat percentages and milk yields of close relatives, such as full sisters, half sisters, and mother and daughter, are presented in considerable detail. Then the influence of the more distant ancestors, such as paternal and maternal grandsire and granddam, on production are taken up. The inclusion of prediction formulae by the use of which the probable performance of a cow can be estimated from the record made by her dam, full sister, or half sister are valuable.

One chapter is devoted to assortive mating in the Holstein Freisian breed and one to the relative influence of environment and heredity on milk yields and butter-fat percentage. The proportionate effect of measured heredity versus environment depends on the nearness of the relatives. In any cow heredity probably plays the greater part in the permanence of milk yield or butter-fat percentage.

The final chapter gives a resumé of the data on the inheritance of milk yield and butter-fat percentage. It is interesting to find in this chapter a comparison of the actual correlation found between milk yields of relatives with what might be expected from the theoretical distribution of common chromosomes.

Unfortunately, the book contains several typographical errors none of which, however, interfere with its usefulness.

MANUAL OF DAIRY CATTLE BREEDING, by JOHN W. GOWEN. 112 pages. Price $3.00. The Williams and Wilkins Company, Baltimore, Md. 1925.

The manual is a laboratory guide prepared for use in connection with the author's book on Milk Secretion. It is intended for students interested in dairy cattle breeding and who have had training in the elements of statistics and principles of heredity.

The problems selected for the students' use are taken from chapters in Milk Secretion and the sequence of subjects in the two books is much the same. Each chapter includes directions and suggestions on the particular problem together with blank forms to be filled in and suitable references in which the student can obtain additional information. In order to satisfactorily complete the work it would be necessary for many references to be consulted, and this is a very valuable feature of the manual, since it brings the student in contact with original literature.

H. C. McPhee.

NEWS AND NOTES

Mental Classification of Prisoners in Massachusetts

IN compliance with a law passed in 1924, requiring mental examination of all prisoners in Massachusetts, the Department of Correction has completed the classification of the first 1,500 cases. These are divided into four groups: The mentally affected (281); the alcoholic (617); the socially maladjusted (449); miscellaneous (153). In the first group 43 were considered to be suffering from mental disease, seven were recommended for commitment to a school for the feeble-minded; 79 were classified as defective delinquents, and 152 as psychopathic delinquents. The mental defectives thus comprise only about six per cent of this group—very much less than the percentage alleged by some writers. The psychopathic delinquents furnish a large proportion of the "repeaters," and "indeterminate commitment to a special institution seems to offer the only promise of successful treatment."

"In 1924 Massachusetts found that 48.6 per cent of the inmates of the House of Correction were known to have several previous sentences, of this number 19.5 per cent had been sentenced from six to fifteen times."

DR. W. OVERHOLZER,
Mental Hygiene Bulletin.
April, 1926.

Idaho State Eugenic Board Holds Initial Meeting

IDAHO'S state board of eugenics met in Boise Thursday for the first time since its formation by the 1925 legislature. Immediately after convening the board went to Nampa, where it surveyed the institution for the feeble-minded, to see if its good offices were needed there.

This morning it will reassemble in the statehouse, and will organize by election of a chairman and transact other routine business.

Governor C. C. Moore accompanied the board members to Nampa. Others present, all of them members of the board, are Dr. F. M. Mitchell, superintendent of the Blackfoot asylum; Dr. John I. McKelway, superintendent of the Orofino asylum; Dr. D'Orr Poynter, superintendent of the Nampa institution; Dr. Ralph M. Fouch, state medical adviser; J. W. Wheeler, warden of the state penitentiary, will attend today's meeting. W. D. Vincent, superintendent of the St. Anthony school, also is a member of the board. He did not attend Thursday's meeting, but may be here today.

David Burrell, commissioner of public welfare, is assisting the board in its preliminary work.

—*Boise* (Idaho) *Statesman.*
May 21, 1926.

Minnesota Eugenics Society Formed

THE Minnesota Eugenics Society which is open to membership to any citizen of the state, today filed articles of incorporation in the office of Mike Holm, Secretary of State.

There is no capital stock in this corporation, and it will not operate for profit for the corporation or its members.

The purpose of the society is to "spread abroad knowledge concerning the laws of heredity and the principles of eugenics, to promote race betterment,

and to obtain the passage and enforcement of eugenic laws to that end."

The organization will have its headquarters in Minneapolis.

First officers are Dr. Charles F. Dight, Minneapolis, president; Dr. George G. Eitel, Minneapolis, first vice president; W. A. Laidlaw, St. Paul, second vice president; Dr. W. E. List, Minneapolis, secretary, and C. A. Quist, Minneapolis, treasurer.

—*St. Paul* (Minn.) *Dispatch*.
May 12, 1926.

Virginia Sterilization Act Held Constitutional

(*Buck V. Bell* (*Va.*), 130 *S. E. R.* 516)

THE Supreme Court of Appeals of Virginia says that the appelant, by her guardian and next friend, complained of a judgment of the circuit court by which Dr. Bell, superintendent of the State Colony for Epileptics and Feebleminded, was ordered to perform on her the operation of salpingectomy, for the purpose of rendering her sexually sterile. At the time she was committed to the colony she was 17 years old, had the mind of a child 9 years old, was the mother of an illegitimate child of defective mentality, and her mother had theretofore been committed to the same colony as a feeble-minded person. Unless sterilized by surgical operation, she must be kept in the custodial care of the colony for thirty years, until she became sterilized by nature, during which time she would be a charge on the state. If sterilized under the law, she could be given her liberty and secure a good home, under supervision, without injury to society. Her welfare and that of society would be promoted by such sterilization.

The judgment complained of was rendered by the circuit court on an appeal from an order of the special board of directors of the colony directing that the appellant be sterilized by the surgical operation of salpingectomy, the superintendent of the colony having presented a petition for such order. The proceeding was strictly in conformity with the statute. But it was contended that the judgment of the court was void because the Virginia sterilization act was repugnant to the provisions of the state and federal constitutions in that it did not provide due process of law, imposed a cruel and unusual punishment, and denied the appellant and other inmates of the state colony the equal protection of the law. However, the act complies with the requirements of due process of law. An adjudication by an impartial tribunal, vested with lawful jurisdiction to hear and determine the questions involved, after reasonable notice to the parties interested and an opportunity for them to be heard, fulfils all the requirements of due process of law. Nor could the contention that the statute imposed cruel and unusual punishment be sustained. The act is not a penal statute. The purpose of the legislature was not to punish but to protect the class of socially inadequate citizens named therein from themselves, and to promote the welfare of society by mitigating race degeneracy and raising the average standard of intelligence of the people of the state.

—*Jour. Amer. Med. Assoc.*
January 3, 1926.

Eugenics Meeting of Commonwealth Club of California

THE monthly dinner meeting of the Commonwealth Club of California, held April 13, was devoted to a discussion of Eugenics. It was one of the most successful in the history of the club. About 300 people were present representing the eugenically worth while group of California.

They were big men and accustomed to getting things done. It seemed to me they were very thoughtful as they left the meeting.

This same thing could be done all over the country. One cannot help but think of the time when the Rotary Clubs were really accomplishing little beyond good fellowship. Then there came to their national presidency a man fired with the spirit of social service that his influence radiated throughout, not only the Rotaries, but eventually all the service clubs.

C. M. GOETHE.

Thyroid-Induced Moulting in Fowls

WRITING in *Archiv fur Entwicklungsmechanik*, Vol. 107, pp. 329-353, B. M. Zawadovsky reports the results of experiments in feeding thyroid to fowls. About 100 fowls were used, and the experiments extended over five years. He found that when fowls were fed a single large dose of fresh thyroid (from horses), their feathers began to drop out after from seven to thirteen days. This induced molt was in some cases nearly complete, so that the fowls lost most of their feathers. After the molting process was well under way, new feathers began to grow, but the new feathers contained less pigment than the old, being paler and containing more white or unpigmented spots. These two results of (1) depluming and (2) depigmentation seemed to be quite regular consequences of heavy thyroid feeding. They could be induced also by daily feeding with smaller doses of thyroid, and by injection of thyroxin, the active principle of the thyroid secretion. The molt in fowls is usually seasonal, taking place in the fall; but under Zawadovsky's methods, it can be induced at any time.

Zawadovsky suggests that the normal fall molt is correlated with the normal increase in thyroid secretion at this season. Here two characters of fowls which have been regarded as chiefly determined by heredity—i. e., the molting process and the pigmentation of the feathers—are found to depend as well on other factors which can consciously be altered. Chiefly the results invoke a new respect for the inner secretions, for here is a new way in which they affect the characteristics of the animal.

Effect of Heredity and Environment in Determining Color of the Himalayan Rabbit

IT has been known for some time that the pattern of the Himalayan rabbit (pink eyes, white coat, dark ears, nose, tail, and feet) depends on (1) a single factor allelomorphic with albinism, and (2) the temperature in which the rabbit is kept. If white hairs are pulled out and the animal kept in the cold, the new hairs come in black. (For further details see "Varieties of White Rabbits," W. E Castle, *Journal of Heredity*, Volume 15, 15, Page 218, 1924).

N. A. Iljin has recently studied the temperatures at which the color change takes place. (Volume 1 *Transactions of the Laboratory of Experimental Biology of the Zoological Park of Moscow*) He finds that it differs for different parts of the body, and most important of all, it differs in different races of Himalayan rabbits. Some races show the reaction at a slight lowering of the temperature, and others have to be subjected to extreme cold to bring it about. Iljin shows that what is inherited in Himalayan rabbits is not a pattern, but a specific type of reaction of different tissues to specific environmental conditions. The same genes determine white at one temperature and black at another; while other

genes alter the threshold of response; i. e., raise or lower the dividing line between a "black" temperature and a "white" one.

Thus it is possible within limits to alter a character either by altering the environment or by altering the genetic constitution through breeding. This double control increases the number of possible kinds of expression of the character, for each new environmental factor which becomes known has much the same effect as a new set of genes discovered; i. e., the number of possible combinations is increased not merely arithmetically but geometrically.

Both Iljin's work with Himalayan rabbits and Zawadovsky's are in the direct line of the new advances in genetics, which attempt to determine how the genes produce their effects and how they are fitted into a scheme of development which is the resultant of both internal and external forces.

L. C. D.

U. S. To Enter for First Time in World's Poultry Congress

THAT the world's greatest poultry industry—that of the United States—will be represented at the World Poultry Congress and Exhibition at Ottawa, Canada, in 1927, is now practically assured, says Dr. M. A. Jull, in charge of poultry investigations, United States Department of Agriculture. The passage of the bill last week by both the Senate and the House makes possible this country's participation. It remains only for the department to secure the necessary appropriation.

It will be the first time the United States has participated in the world event of which this is the third. The first congress was held at The Hague in 1921 and the second at Barcelona, Spain, in 1924. Twenty-five countries have already made application for entrance in the Canadian show.

The United States is the leading poultry country, having more than one-third of the world's poultry population. China comes second, with about 16 per cent of the total.

The poultry industry in the United States ranks sixth in value of products, being exceeded only by dairy products, corn, cotton, hay and forage, and swine. It is easily a billion-dollar industry. Not only is it important from a monetary standpoint, but also in point of numbers of people directly and indirectly interested. More people are directly interested in raising poultry than in the production of any other class of livestock or crop. It is estimated that poultry is raised on 90 per cent of the farms in this country, many of which report that it is one of the best-paying crops. Indirectly from 75 to 90 per cent of the people are interested in poultry from the consumption standpoint.

The exhibit to be prepared under the direction of the United States Department of Agriculture will portray the fundamental features of the industry and its importance as an agricultural crop in the United States.

Population Conference Postponed

The International Conference of Population Growth at Geneva, planned to take place this summer, has been postponed. It is quite likely that it will be held in the summer of 1927, but no definite plans have yet been made.